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Art. XIV.—*A new Theory of Vision;* by Samuel Rowley, M.A.

My purpose in the following communication is to offer a theory of vision which I believe has not been heretofore advanced, and which I venture also to believe to be the true one. But before entering upon its consideration it may be proper briefly to state, with reasons for rejecting them, the theories hitherto advanced.

The theory of Aguilonius supposes that all the images, impressed on the retinæ at any given instant, are seen in a plane perpendicular to the plane of the optic axes at their point of convergence, and parallel to the line joining the centers of the two pupils; and consequently that the two images of any object situated in this plane, occupying as they necessarily do one and the same place, are seen single with two eyes.

If any person whom a little practice has fitted to make such experiments, will place at some short distance, say three inches, in a line parallel to the eyes, two small objects, one directly before the central point of the interval between the eyes, and the other at a distance from this of say $\frac{3}{4}$ of an inch, and direct his optic axes to the former, he will not see the two images of the latter as one, though their object is situated in the plane mentioned, but as two distinct images, the one beyond the other.

Experiments to the same effect might easily be multiplied.

But the one given abundantly demonstrates the inaccuracy of the theory of Aguilonius.

The theory of *corresponding points*, so called (as held on physiological grounds by Galen, Alhazen and Newton,) cannot be denominated a theory of vision. It is directed to the accounting for but one of the phenomena of vision—that of single sight with two eyes. It does not deal with visible direction and distance; whereas these conditions, governing, as they do, external visible position, include the result of single sight with two eyes as a necessary consequence.

The theory of vision propounded by Dr. Smith in his Optics, and that by Dr. Wells, from which he derives his explanation of the phenomenon of single vision with two eyes, in his "Essay on single vision with two eyes," are practically (as I trust will satisfactorily appear hereafter) merely attempts to formulate the misapprehensions by consciousness of true visible distance which take place under the influence of habit. A simple experiment subversive of both these theories may be made thus. Let two small objects, as pins, in a board, be placed directly in front of the middle point between the eyes, the one at some short distance, say six inches, and the other at some more considerable distance, say eighteen inches, and let other two small objects be placed on opposite sides of the near one in lines between the eyes and the far object, and the optic axes be directed to the near object. Then the two images of the remote object will be found at no greater distance from each other than the images of the objects at the sides of the near one, or all will be apprehended as lying in the figure of a rectangle.

But by the theory of Dr. Wells* the two images of the remote object (since this object lies in the direction which he has named the common axis) are seen in the optic axes—the right eye's image in the left eye's axis, and *vice versa*—and at a distance from each other (if located at the same distance from the eyes as the object)† of $4\frac{3}{10}$ inches, whereas the images of the two lateral near objects, since these occupy the line named *horopter*, will be seen in the places of the objects at a distance from each other of $1\frac{6}{10}$ inch.‡

As to the two images of any object more or less distant than the convergence of the axes, the theory of Smith places them in lines drawn from the eyes to the object somewhere between the place of the object and the point of convergence, "but not very far from the real place," i. e. the place of the object.§ Hence, while the two images of the object on either

* Essay upon Single Vision with Two Eyes, p. 38. London, 1818.
† Ib. page 27.
‡ Ib. pages 5 and 46.
§ Smith's complete System of Optics, Art. 137.

side of the point of convergence will be seen coincident with their object at a distance of $1\frac{6}{10}$ inch from the two images (seen in like manner) of the object on the other side, the two images of the remote object will be seen at a much less distance from each other, or, following the proportions of the construction given us by that author, at a distance of $\frac{5}{10}$ or $\frac{6}{10}$ inch.

Aguilonius is the only author who, meeting the question in its full extent, undertakes to assign for each image both the line of visible direction and the absolute visible distance from the retina on the line of visible direction.

Sir David Brewster indeed gives both conditions of external visible position for the images falling *on the vertices of the retinæ*. But for every other image he merely assigns Kepler's line of visible direction, being silent regarding the visible distance—the essential complement of the former in determining visible position.

I will now propose and proceed to put to the test of experiment, what I believe to be the true theory of vision; premising the remark that in establishing its existence it will not render the proof offered less exact if I do not undertake to determine the precise point where a given ray* strikes the retina nor the precise point at which the line of visible direction crosses the optic axis, provided we admit, (what I think will not be disputed) that where the angles with the optic axes of two rays, falling one on each cornea, are equal, the distances of the images on the retinæ from the extremities of the axes are equal, and provided we admit, (what I also think will not be disputed) that where the angle with the axis of one of the rays is but *very slightly* increased, the angle formed after decussation is not appreciably *un*equally increased; and further premising that I will neglect any effect of refraction and also suppose a coincidence of the line of the ray with the line of visible direction.

All the component points of the total impression on the retina of either eye, before becoming cognizable mental phenomena, are simultaneously and in lines drawn from them through a point situated, say a little behind the crystalline lens, referred outward to a surface, parallel to the impressed surface of the retina conceived to be simply expanded coextensively with the impression as expanded by outward reference and cutting at right angles the optic axis of that eye at the point where it intersects the optic axis of the other eye.

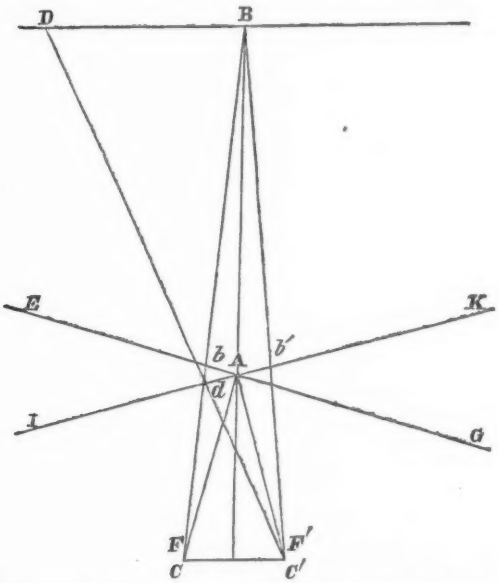
* Here and wherever in this paper the word *ray* is used, it is intended to signify the central or axial ray of the pencil.

Or, to change the form of statement, *the entire impressions on the retinæ, before becoming objects of consciousness are projected in space upon surfaces bisecting each other (at an angle greater or less according to the distance) in a plane perpendicular to the plane of the axes—the component points of each impression being simultaneously referred outward in lines passing from them through a point a little behind the center of the crystalline lens, but, excepting the expansion and the inversion resulting from the crossing in the eye of the directions of outward reference, undergoing no change of relative position—the distance between the planes passing at right angles to the optic axis through any two of the successive concentric zones of points, which make up the retinal impression, continuing the same.*

I do not mean here to assert that an outward projection actually takes place, but that the *effect* of such a projection, produced by an intermediate process of mind, is presented to the consciousness.

On a suitable plane surface take two points C, C′ (fig. 1),

1.



$2\frac{4}{10}$ inches apart, and in a line bisecting at right angles one joining these points take other two points A, B, distant respectively from the point of bisection six and eighteen inches.

Draw the straight lines C A, C′A, and perpendicular to them at A the two lines EG, IK. Through the points F, F′, taken in the lines CA, C′A, $\frac{1}{10}$ inch from C, C′, draw the lines FB, F′B, intersecting EG, IK, at *b*, *b*′. Through the points F′ and *d*,—a point in IK where a line passing through *b* perpendicular to CC′ intersects it—draw a line. In this line, where a line passing through B parallel to C C′ intersects it, take the point D. The distance of this point from B will be $4\frac{9}{10}$ inches.

Now suppose the points C, C′, to represent the centers of the eyes; points F, F′, the focal centers; A, B, D, the situations of luminous points; the lines CA, C′A, the optic axes produced; the lines EG, IK, lines of intersection where two surfaces slightly convex toward the eyes, perpendicular respectively to the optic axes at their point of meeting, cut the plane of the axes; BF, BF′, lines of luminous rays passing from the point B through the points F, F′; DF′ the line of ray passing from D through F′ to the retina; *b* the point where the line BF intersects the line EG; and *d*, the point of intersection of DF′ with IK. Then, if the above enunciated proposition be true, to wit: that an impression made by a luminous point on the retina of the right or left eye becomes visible and is seen where a line drawn from it through a point a little behind the center of the crystalline lens pierces a slightly convex surface perpendicular to the optic axis of that eye at the place of its meeting with the axis of the other eye, the image of the point at B, will be seen from the left eye at *b*, and the image of the point at D from the right eye at *d*, both in the same perpendicular of no variation,* or the image of the point at B will be seen by the left eye a little behind the place in which the image of the point at D will be seen by the right eye.

The experiment may be made thus. Take a thin rule, (as a plane scale) twelve inches in length with the right hand, and another rule with the left hand. Hold the rule in the right hand with its wide sides perpendicular to the plane of the horizon, in a line running away from the middle point between the eyes so that the near end shall be distant six inches and the remote end rest upon a horizontal bar of a well lighted window at right angles to the rule, and parallel to the eyes.

To obtain the former distance with accuracy it is necessary to have two small objects, as two pins, placed equidistantly on opposite sides of the point of intersection of the bar by the rule, distant from each other $4\frac{8}{10}$ inches, and when these shall be seen across (or lie in the axes exactly fixed upon)

* By perpendicular of no variation is meant a line perpendicular to the base of vision, for the definition of which see note, p. 164.

the near end of the rule, that distance for eyes $2\frac{4}{10}$ inches apart will be accurately found. Now fix the axes of the eyes on the near end. The images of the remote end will appear as two at the sides, that of the left eye at some unknown distance on the left, and that of the right eye in like manner on the right. Holding the rule in the left hand in the same position with respect to the plane of the horizon as the other, apply its remote end on the window bar to the left of the remote end of the rule in the right hand, so that the right eye's image of the former will cover the left eye's image of the latter. Now measure the distance between the far ends on the bar. If all the conditions of the experiment shall have been exactly complied with the distance found will be $4\frac{9}{10}$ inches, as predetermined in the diagram.

The experiment, then, shows that the geometrical consequence of the principles of the proposition, as illustrated in the diagram, holds good in actual practice, and that when the axes are fixed upon the images of an object placed as a luminous point at A, the left eye's visible image of an object placed as a point at B in the line FB, is seen a little beyond where the right eye's visible image of an object placed as a point at D in the line drawn through F′ and *d* is seen, both in the same line of no variation. It is obvious by the proposition that it ought not to affect the "conjunction" of the images, wheresoever in the line passing through F′ and *d* the object might be placed; and this will be found to be the fact. It was placed opposite B at D merely for convenience of applying the diagram in actual experiment, and to procure a correct appreciation of the interval between the images.

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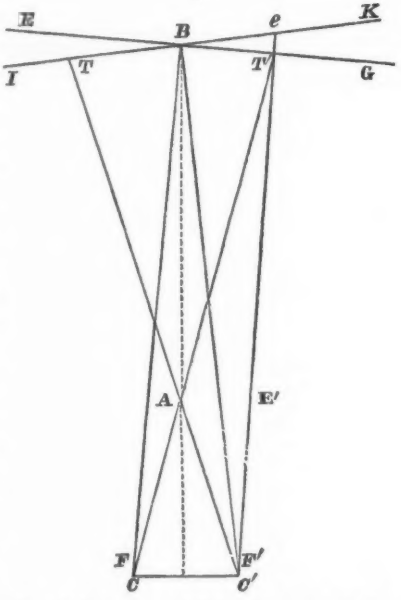


Suppose in the second place, the axes represented by CB, C′B, fig. 2. Then by my proposition the left eye's visible image of the

luminous point at A should be seen at T′ in a surface cutting the axis FB at right angles in B (and the plane of the axes in the line EG) and the right eye's image at T.

But if the left eye's image of the point at A is seen at T′ then by the same proposition the right eye's image of the point at E′ opposite A in a line between F′ and *e*, a position in IK, directly beyond T′, should be seen in the same line of no variation a little beyond T′ at *e*. If the experiment be made it will be found that the right eye's visible image of an object placed as a point at E′, nearly $1\frac{6}{10}$ inch from A, will be seen a little behind the place where the left eye's image of an object placed as a point at A is seen.

So the two middle images of the pins placed as a test of distance in the first of these experiments are seen united at the intersection of the surfaces of vision.

If the surfaces of vision were planes, since their line of intersection would then coincide with the line of intersection of the vertical planes of the axes, it is evident that the images of these pins longitudinally bisected by the latter planes would exactly coincide. But owing to the slight curvature of the surfaces of vision, the line of intersection of these surfaces immediately begins very gradually to recede from that of the vertical planes of the axes, and the longitudinal axes of the images of the pins to cross over and fall on opposite sides, thereby preventing a mathematical coincidence of the images.

This effect of the departure of the intersection of the surfaces of vision (supposing the optic axes parallel to the horizon, to which the pins are perpendicular, to be directed to the bottoms of the images) may be counteracted and a mathematical coincidence of the images nearly produced by inclining the pin whose image is seen by the left eye a very little to the left and the other duly to the right. Should this small inclination be given with such a curve that a line drawn from any point of the axis of the pin to the focal center of the eye would intersect the line of intersection of the surfaces of vision, then an exact mathematical coincidence of the images would be secured.

The approximate result may be obtained if two rulers* or the legs of a pair of compasses† be so placed that their longitudinal axes shall lie in the vertical planes of the optical axes.

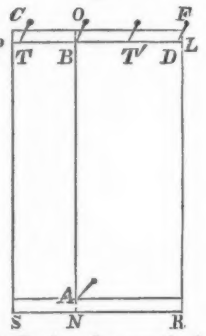
It is evident that for the two objects in the vertical planes of the axes, we may put a single object at the intersection of these planes.‡

* See Wells, p. 36. † Smith's Optics, Art. 977. ‡ Wells, p. 49.

But to make the accurate performance of these experiments easy, some special instrument must be provided. A piece of pine board whose opposite great surfaces are rectangles parallel to one another, thirteen inches long and eight inches wide, with five pins, furnishes the means for obtaining accurate results, where the distance between the near and far points does not exceed twelve inches.*

To use this contrivance—supposing the distance from the middle of the line between the centers of the eyes to the near objective point to be six inches, and from the near to the far objective point twelve inches—in the surface SRFC (fig. 3) $\frac{1}{2}$ inch from the short side SR, and five inches from the long side RF, take the point A. Through this point and at right angles to the side SR, draw a line NO. In this line, twelve inches from the point A, take the point B. Through this point, and at right angles to this line, draw another line PL. In this latter line, take the two points T, T′, each distant $2\frac{4}{10}$ inches from the point B, and $4\frac{8}{10}$ inches from the other; and finally $4\frac{9}{10}$ inches from the point B, take the point D. At each of the points, so taken, set a pin. Now place the board in a horizontal position a little below and parallel to the plane of the axes, with the pin at A six inches from the middle of the line joining the centers of the eyes, and, together with the pin at B, in the vertical plane which bisects this line at right angles; which disposition of the pins is had, when, looking at the images of the pin at A, the left eye's image of the pin at T′, and the right eye's image of that at T, are found lying in the axes.

3.



When now the axes are fixed upon the images of the pin at A, the right eye's image of the pin at B will be seen in the same line of no variation as the left eye's of the one at D, and at a point a little beyond where the latter is seen; and when they are fixed upon the images of the pin at B, the left eye's image of that at A will be seen in a position a little before where the right eye's image of a pin, set nearly $1\frac{6}{10}$ inch directly to the right of A, will be seen, the distance between these being sensibly less than that between the images received from B and D, when the axes were converged toward A.

* Another board of the same width and 18 inches long serving as a support to this, to be held with one end against the nose by the hands pressing both pieces at the sides, will be found serviceable for maintaining steadiness of position.

A still better instrument, is a small wooden cross consisting of a slender bar and sliding transverse piece, which may be any slip of wood, (as a piece of common lath) notched at the center so as to receive the longitudinal bar in its full thickness at right angles, and also capable of being shifted and fixed by wedging at any point.

For all experiments in which the distance of the remoter of the two stations designated A and B, figs. 1 and 2, is not greater than 18 inches, the length of the bar should be 18 inches, and that of the transverse piece, 12 inches.

To apply this instrument instead of the window bar and rulers in the first of the examples taken, fix the transverse piece at the distance of 17 inches from one end of the shaft, and laying the cross on a table with its level side upward, draw on the upper side of the shaft lengthwise the bisecting line NO, and lengthwise on the upper side of the transverse piece, the bisecting line PL. At B, the point of intersection of these lines, set a pin, and at the point A, 12 inches from the point B, in the line NO, also one. At the points T, T′, in the line PL, each distant $2\frac{4}{10}$ inches from B, set two other pins, and at D, $4\frac{9}{10}$ inches from B, another. Then placing the cross so as to lie with the extremity N, of the shaft against the upper part of the nose, and its upper surface a little below and parallel to the plane of the axes, and keeping the head fixed, look with each eye in rapid succession at its image of the pin at A. If the right eye's image of the pin at T, and the left eye's of that at T′, be seen in these directions of the axes, the pins at A and B are 6 and 18 inches distant from the middle point of the line joining the centers of the eyes. But if they should fall within or without the axes thus directed, then the transverse piece, together with the pins at A and B, 12 inches apart, must be shifted slightly forward or backward, until the requisite position is found.

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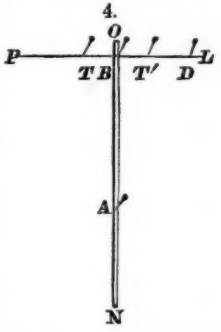


Holding this instrument in the same position with respect to the eyes as that just above given to it for ascertaining distance, turn it toward a strongly illuminated white wall or sheet of white paper or (which is better) the clear sky. If now the axes be fixed upon the images of the pin at A, the right eye's image of the pin at B, and the left eye's of that at D, will be seen lying in the same line of no variation, the former a little beyond the latter. But if having transferred the transverse piece to the

distance of A, the axes be fixed upon the images of the pin at B, then the image of the pin at A, seen by the left eye, and that of a pin placed nearly $1\frac{5}{7}$ inch directly to the right of A, seen by the right eye, will be found lying in the same perpendicular of no variation, the latter beyond the former, the distance between the two images being less than in the first case.

In experimenting with this instrument, the position of the image of the pin at B seen by the left eye in a surface at right angles to the optic axis of that eye is visibly realized. For when the axes are fixed toward A (as in the first instance) the image of the transverse piece seen from the left eye visibly has a position at right angles to the axis of that eye, and intersects the corresponding image of the right eye, in a line of common intersection crossing each image where that image, whether referred back to the retina or to the distance of the transverse piece itself, would be pierced by the optic axis.

In making these experiments, care should be taken not to turn the axes into the oblique directions, to do which there is always a strong inclination; for then the images, which were seen, in those directions as described, the one before or behind the other, would be seen coincident.

I will now proceed to answer an apparent objection. In the first demonstration, under my proposition, I undertake to show that the impression on the left eye from B becomes visible and is seen at *b*, by showing that it is seen a little behind where the impression on the right eye from an object placed at D is seen by the right eye, arguing that nothing else than a projection in the manner described can account for the local relationship in which these impressions are seen. The evidence of consciousness, then, that these impressions are seen in a certain local relationship, is made the groundwork of my argument. Now, it may be urged that the evidence of consciousness is equally strong, that the impression from the object at B on the left eye, when seen, lies in the axis of the right eye at the actual distance of the object itself at B.*

In the first place, let us determine whether consciousness, when carefully examined, does really continue to find this impression elsewhere than at *b*. To reveal in consciousness the true distance in a lateral direction of the visible image at *b* of an object at B (fig. 1), employing the pins and board, place a pin at *b*. The left eye's image of the pin at B will not be recognized to the left of the left eye's image of it.

Again, take three equal, small rings of pasteboard. Attach to each, in the direction of a radius produced, a pin, by cement-

* See Wells, pp. 38 and 27.

ing with a drop of sealing wax the head of the pin to the edge of the ring.

At B, fig. 3, (the board duly widened,) a point $4\frac{8}{10}$ inches to the right of B, and a point $4\frac{8}{10}$ inches to the left, station these pins, so that the rings shall be parallel to the eyes of the observer. At the points A, T, T′, set simple pins. Now on converging the axes toward A, till the right eye's image of the pin at T and the left eye's of the pin at T′, are hidden in the combined images of the pin at A, the images of the rings will be apprehended in their true visible places.

To make another experiment of the kind to which the last belongs, on a glass of a window situated at a convenient height, stick a number of wafers, placing their centers in a horizontal line and separating the adjacent centers by a distance less than that between the centers of the eyes, but being extremely careful to make all these distances exactly equal. Direct the axes through the centers of two adjoining wafers, each axis through that center lying on its own side, and presently all the visible images (except usually the left terminal image of the right eye's series and the right terminal one of the left eye's), will be recognized by consciousness in their true visible positions.

If the adjoining centers of the wafers are very close to each other (as a little over $\frac{1}{2}$ inch, supposing the wafers to be $\frac{1}{2}$ inch in diameter) and the eyes at a short distance, say 4 inches, *the curvature of the surface of vision will be plainly appreciated*, and if the plane of the axes be turned on the line joining the intersection of the axes to the center of the interval between the eyes from a coincidence to an angle with the horizontal plane passing through the centers of the wafers, *the curvature of each eye's series will be separately appreciated.*

An experiment of the same kind may also be made by means of the similar figures lying at equal distances in the surface of a papered wall.

If the experimenter will station himself facing such a wall and converge his optic axes, so that the axis of his left eye shall pierce the center of some figure in front of him and that of his right eye the center of the next one to the left of the other, then shortly each eye's image of the papered wall will, to his consciousness, take its true visible position at the distance of the convergence of the axes—the images of the figures appearing smaller or larger according as the distance of the point of convergence is less or greater. A variation (from the standard) of the distance between the centers of the figures of the wall, will produce a seeming elevation or depression of the visible images. The cause of this last as well as that of the principal phenomenon, will be apparent, when it shall have been

explained why the lateral images of an object which is at a greater or less distance than that of the convergence of the axes, are not apprehended in their true visible positions in the surfaces of vision.

Why then does consciousness mistake, precisely as we find it doing, the true place of the visible image of any object, lying not at the point of intersection of the axes nor at any point equally distant with this from the base of vision ?*

My answer is, that under tendencies resulting from experience, consciousness apprehends erroneously the distances really presented for its action according to the laws of vision, and with respect to each lateral image apprehends the distance of the surface of vision from the base of vision, and the distance in the surface from the image at the center for the distances that would exist between these surfaces and these images, if the given image, now laterally and obscurely seen, were with its corresponding image of the other eye directly and distinctly examined at the center.

Hence the place in which such an image seems to be seen.

For example, let the axes be turned to the images of an object at B, fig. 2. They will be seen at B, the left eye's $2\frac{4}{10}$ inches, to the left, from its image at T′ of the object at A, and the right eye's $2\frac{4}{10}$ inches, to the right, from its image at T of the object at A, each in a surface of vision, measuring from the apex, 18 inches from the base of vision.

Let now the axes be turned to the images of the object at A, fig. 1. These will now be seen at A, that of the left eye's $\frac{8}{10}$ inch to the right, from its image at *b* of the object at B, and the right eye's $\frac{8}{10}$ inch to the left from its image at *b*′ of the object at B, each in a surface, measuring from the apex, 6 inches from the base of vision.

At first then, the left eye's image of an object at B when particularly examined, will always be apprehended at the distance of $2\frac{4}{10}$ inches, to the left, from the image at T′ of an object at A in the surface of vision 18 inches from the base.

Hence after due experience will arise a tendency of mind to apprehend this image at a distance of $2\frac{4}{10}$ inches, to the left, from the image of the object at A in the surface of vision 18 inches from the base. Therefore, when the axes are turned toward the images of the object at A, the mind will be influenced by this tendency, with respect to the left eye's image at *b* of the object at B, to apprehend 6 inches, the distance of the surface of vision from the base, as 18 inches, and $\frac{8}{10}$ inch, the

* By *base of vision* I mean a plane cutting perpendicularly at the center of the interval between the eyes, a line drawn through this point and the point of convergence of the optic axes.

distance in the surface from the image at A of the object at A, as $2\frac{4}{1\,0}$ inches.

Again, the effect of experience in regarding directly the images of the object at A will be a habit whereby, when the axes are directed to the images of the object at B, the mind will be led to mistake $2\frac{4}{1\,0}$ inches—the distance in the surface of the left eye's image at T′ of the object at A to the right of its image at B of the object at B—for $\frac{8}{1\,0}$ inch, and 18 inches—the distance of the surface of vision from the base—for 6 inches; and $2\frac{4}{1\,0}$ inches—the distance in the surface of the right eye's image at T of the object at A to the left of its image at B of the object at B—for $\frac{8}{1\,0}$ inch, and 18 inches—the distance of the surface of vision from the base for 6 inches.

Thus since the more remote parts of a solid object will seem to be seen at equally more remote distances in the image, and the entire image at a distance equal to that of the object, the image will seem to be seen with the same form and magnitude as really pertain to the object.*

We are now also prepared to explain the cases in which the visible image, though the object be situated at a greater or less distance from the base of vision than the point of convergence of the axes, is accepted by consciousness in its true place, as in the experiments with the equidistant pins, with the wafers upon glass, and the figures in the papered wall.

In these cases it will be perceived that any two images seen as one, though proceeding from two different objects, have the same positions in the surfaces of vision as they would if they proceeded from a single object placed at the intersection of the lines drawn from the two different objects to the eyes.

Further, so great is the propensity to appreciate singly, that any slight discrepancy between these images and those produced by one and the same object will not prevent an acceptance of a correspondence, and, since all the equivalent single objects would lie at the same distance, no habit can exist to influence the mind with respect to any images to locate the surfaces of vision at a greater or less distance from the base of vision than the true. Hence all the images will be apprehended in their true visible places. But if the interval between any two objects of such a series should differ from the other intervals, then for the two (of the four images presented) which are accepted as corresponding, or capable of being seen singly in direct vision, if these should be lateral, or if central, then for all the rest, from the necessary principle of habit above laid down, the surfaces

* In ordinary lateral vision, (i. e. vision had at the sides of the line of intersection of the surfaces of vision), two corresponding images or the corresponding parts of two complex images at the concurrence of the axes are appreciated singly by reason of inattention to, or neglect of, one of them.

of vision, measured from the common center, will seem to exist at such a distance from the base of vision as that at which they would it these images were directly seen in the axes.

In this experiment with the papered wall, with every movement of the head a movement of all the images in the same direction takes place, because with the eyes fixed toward any two figures, a movement of the head in any direction will carry in the same direction all the points of intersection of the lines of direction and consequently all the images in the two surfaces. So when the axes are made to pass through any two of a series of equal and equidistant objects to a point beyond, as in the case of the experiment with the wafers upon glass, a movement of the head in one direction will produce a movement of the images in a contrary direction.

There are other circumstances incident to a lateral image in the surface of vision, besides distance from its twin, which will produce habits operating to defeat a correct appreciation of the two distances.

Such a circumstance is the visible magnitude.

Thus, whatever the visible magnitude of an aggregate lateral image, which has been often seen in direct vision, this magnitude will be taken for that found on particular examination, and the visible distance for that corresponding to that magnitude. But since lateral images in the surface of vision, not distinguishable from each other by consciousness may result from objects of very different magnitudes placed at suitably different distances, a given magnitude of image will not always excite the same tendency of mind.

To illustrate this, take the board with pins placed at A and B (fig. 3,) depress the far end till the surface is hidden by the near end, and, stopping the light flowing from the near pin to one of the eyes by interposing near the eye an adequate object, fix the axes of both toward the remote pin. As soon as the eye has ceased its alternations of adjustment (which have been performed so rapidly as to keep up a sensibly sustained distinctness of both images), and no longer experiences the sensation of focal adjustment to the near pin (which has excited a tendency of mind to mistake the image at the distance of the object), the now shadowy image of this pin will take its position beside that of the far-pin. This it will generally do slowly, because the image in the surface might have resulted from a pin of due dimensions situated at every intermediate point, but in continuous succession giving up every intermediate position it will finally come to rest in its true visible place. Occasionally owing to the dominancy of some habit, it will seem to take a place at a greater or less distance than the true.

But habits created by magnitude may cause mistakes of visible distance involving also *the image in the axis.*

Thus for example, if the mind has been accustomed to appreciate the images of mountains of moderate magnitudes, thereby tendencies will be impressed upon it to appreciate moderate dimensions with images of this class. Hence when the image of a mountain of extraordinary magnitude is presented (in an ordinary state of the atmosphere), consciousness will be influenced to appreciate the dimensions as less than they really are, and, in order to this, the distance from the base of vision as a duly less one. And the effect on any concomitant images, as of trees or animals, would be a like seeming diminution of their visible magnitudes. For it is plain that if consciousness makes a mistake of distance, then in obedience to the law of habit it will assign that size which the image would really have at that distance, or that effect of lateral distance, or resulting expansion, which the divergence of the lines of direction would give at that distance.

Dr. Abercrombie relates the following instance as having occurred in his own experience. "I remember," he says, "once having occasion to pass along Ludgate Hill, when the great door of St. Paul's was open, and several persons were standing in it. They appeared to be very little children; but, on coming up to them, were found to be full grown persons."

The theory which I have thus advanced, taken in connection with the mistaking practice of consciousness, will, I believe, furnish a satisfactory explanation of all the phenomena of vision—some of the more interesting of which phenomena, as well as the subject of monocular vision, I propose in a future paper to consider.

Hastings-upon-Hudson, N. Y.

ART. XV.—*Fundamental Principles of Molecular Physics;* by Professor W. A. NORTON.

IN a recent work by Joseph Bayma, S. J. Professor of Philosophy, Stonyhurst College, England, in which a new theory of Molecular Mechanics is ably set forth, I find a brief critique of my theory of Molecular Physics, published, originally, in this Journal, and republished in the London and Edinburgh Philosophical Magazine. To this I propose to reply, and at the same time to remark incidentally upon some of the fundamental principles of the theory advanced by Professor Bayma. This can be most briefly and effectively done by taking up the

different objections urged by him in due order, and commenting upon them in succession;—having a care not to dissociate remarks that should properly be presented together. The quotations made will be indicated to the eye by being printed in smaller type.

A great number of scientific men, to give an explanation of calorific, electric, and luminous phenomena, assume that aether pervades all ponderable bodies; whence many of them have come to the conclusion, that every molecule of a body is surrounded by an æthereal atmosphere, the action of which is considered to be the source of those phenomena. Professor W. A. Norton, in a series of interesting articles published in the American Journal, gives a theory of molecular physics, of which the fundamental principle is, that each molecule is formed by an atom of ponderable matter surrounded by two aethereal atmospheres of a different kind. I give his words.

The established truths and generally received ideas which form the basis of the theory, are as follows:

1st. All the phenomena of material nature result from the action of force upon matter.

2nd. All the forces in operation in nature are traceable to two primary forces, viz: attraction and repulsion.

3rd. All bodies of matter consist of separate indivisible parts, called atoms, each of which is conceived to be spherical in form.

4th. Matter exists in three forms essentially different from each other. These are 1st, ordinary or gross matter, of which all bodies of matter directly detected by our senses either wholly or chiefly consist. 2nd. A subtile fluid, or ether, associated with ordinary matter, by the intervention of which all electrical phenomena originate, or are produced. This *electric ether*, as it may be termed, is attracted by ordinary matter, while its individual atoms repel each other. 3rd. A still more subtile form of ether, which pervades all space and the interstices between the atoms of bodies. This is the medium by which light is propagated, and is called the *luminiferous ether*, or the *universal ether*. The atoms, or "atomettes" of this ether, mutually repel each other; and it is attracted by ordinary matter, and is consequently more dense in the interior of bodies than in free space.

5th. Heat, in all its recognized actions on matter, manifests itself as a force of repulsion.

The corner stone of a physical theory of molecular phenomena must consist in the conception that is formed of the essential constitution of a single molecule; understanding by a molecule an atom of ordinary matter, endued with the properties and invested with the arrangements which enable it to exert forces of attraction and repulsion upon other molecules. In seeking for this, the most philosophical course that can be pursued is to follow out to their legitimate conclusions the general principles already laid down. . . . . The conception here formed of a modecule involves the

idea of the operation of the two forces of attraction and repulsion: a force of attraction is exerted by the atom upon each of the two atmospheres surrounding it; and a force of mutual repulsion between the atoms of each atmosphere. These we regard as the *primary forces* of nature, from which all known forces are derived.

These are the capital points of Prof. Norton's ingenious theory. But we think that such a theory contains a great deal of arbitrary assumption. And indeed on what evidence are we to grant that matter exists in three forms essentially different from each other? Then how can we know the existence of atoms of gross matter having a spherical form, and therefore extended though indivisible? Why should we admit two æthereal fluids, which are both repulsive and only differ in subtlety? All this the learned Professor assumes without proof, apparently because it consists of "established truths and generally received ideas." But we say that no one has up to this day established the truth of such propositions. As for "received ideas," every one knows how often questionable notions have been and are received without serious examination, especially when expressed by Professors in a very dogmatic style. Are not a thousand hypotheses received? and do they cease to be hypotheses, although he who makes use of them for building a theory adorns them with the high name of principles.

This is all very plausible, but the objections urged are destitute of any real force. We will first consider the general intimation that the theory rests upon "a great deal of arbitrary assumption." No theory of Molecular Physics can, in the nature of things, have any other foundation than general principles to be regarded as hypotheses that have been rendered more or less probable, either by inductions from observation or by à priori reasonings. Molecular Physics cannot be erected, like Mathematics, upon a foundation known from the first to be eternally sure, that of self-evident truths. Mechanical axioms may exist as mere figments of the mind, and have often risen like bubbles in the minds of speculative philosophers, shone with an evanescent splendor, and suddenly burst at the touch of a hard fact. Our author is another instance of a learned philosopher who has faith in such unsubstantialities, and thinks to substitute them as a proper basis for a theory of Molecular Mechanics, in place of the general conceptions to which the progress of science leads, and by which alone its highest inductions find any explanation—regards the latter as arbitrary assumptions, and his own mental convictions of what matter must be and how it must act, as the only reliable foundation upon which to build. It is true that he takes exception to Principles 3rd and 4th, from the inductive point of view. Upon this ground—the only legitimate one to be occupied—I

am quite ready to meet him: but I wish to enter here, at the outset, a demurrer against the virtual claim of the superiority of his own à priori method of establishing his fundamental principles. Such a claim is implied in the intimation that "no one has up to this day established the truth of such propositions;"—as will be best appreciated by those who have read Prof. Bayma's book. Having proved, as he conceives, his propositions, and clinched each one of them with a Q. E. D., he insists that obvious intimations of Nature are to be discarded because the stamp of infallibility cannot be put upon them at once, before the test of availability in the explanation of phenomena has been applied. It was evident from the tenor of my exposition of the subject that the "established truths" referred to were merely regarded as having been virtually established, or rendered highly probable, by the inductions of science. The claim implied in Prof. Bayma's criticism, that they require a higher confirmation, in fact a demonstration of their truth, is not to be admitted.

He asks:

On what evidence are we to grant that matter exists in three forms essentially different from each other?

A sufficient answer to the critic himself, is that pursuing a systematic course of deduction from his leading principles, and his assumptions of the essential nature of matter, he actually proves to his own satisfaction that matter does in fact exist in essentially three different forms. He reaches the conclusion that every primitive molecule consists of an attractive nucleus surrounded by a repulsive envelop. My own position is that every primitive molecule consists of an attractive atom of gross matter surrounded by a repulsive atmosphere of electric ether. The atom of gross matter answers to his attractive nucleus, the electric ether to his repulsive envelop. The difference of doctrine, from the present point of view, is in name only. In another connection he elaborately undertakes to prove that ether (i. e. the ether of space) is a "special substance." Thus he makes out that there are three essentially different forms of matter.

But to reply to others who may be disposed to adopt the objection urged. No one will deny the existence of gross or ponderable matter; or of something which has all the mechanical attributes of matter. That an ether exists in space and within transparent media, we may certainly regard as abundantly established by optical phenomena. As to the electric ether, the evidence of its existence is that the great body of electric and magnetic phenomena, it is generally

conceded, admit of satisfactory explanation on the hypothesis of an electric fluid, or ether, intimately associated with matter; and that no successful attempt has yet been made to account for the simplest of these phenomena on any other hypothesis. Some physicists, it is true, are striving to do away with the supposed electric fluid;—prompted by the conjecture that Nature must operate by some simpler method, and work out all her wonderful diversity of phenomena by one, or at most, two forms of matter. Shall we wait until these physicists have realized their aspirations, at their discouraging rate of progress; or, guided by the indications of Nature, strive to link all natural phenomena together by a few recognized principles. A theory that shall accomplish this is the great desideratum. Even should such a theory not rest upon the highest and fewest possible mechanical principles, still the generalizations embodied in it must have their counterparts in certain physical truths, to the knowledge of which it will be likely to lead. It is by following the ascending grade of generalizations that speculative science has hitherto progressed. Preconceived notions of what matter must be in its essential nature, or by what form of matter, or varieties of method, Nature must operate have thus far contributed little to its advancement: and in fact when we consider that we positively know and can know nothing, à priori, with regard to the essential nature and condition of matter, and its means and mode of operation, such notions are entitled to little credit.

Our author implies in the remarks above quoted that the existence of an electric ether is not only not an "established truth," but is to be ranked among those questionable notions that have been received without serious examination. This implication is obviously unjust. Besides, the serious examination that he has given the subject only leads him to confirm the substantial truth of what he would here seem to discredit—for, as we have already seen, his "repulsive envelope" is essentially my "electric atmosphere."

Why should we admit two æthereal fluids which are both repulsive and only differ in subtlety.

Prof. Bayma and myself agree in admitting the existence of two *kinds* of matter,—attractive and repulsive; and as we have seen, three *forms* of matter. Is it inherently any less probable that two of these should be repulsive and one attractive, than as he assumes that two should be attractive and one repulsive:—viz., gross matter and the ether of space attractive, and the elements of the "repulsive envelope" repulsive. In the supposition that the two etheral fluids differ in subtlety,

nothing more is essentially implied than that a considerable number of atoms of the one occupy the interstices between the atoms of the other. Prof. Bayma assumes equally great differences to subsist between his two attractive forms of matter. He remarks, "the distinction of such a medium" (a medium for the transmission of light) "from any ponderable substance, is not an hypothesis but a necessary inference drawn from observed facts;" and again, "I do not see how such a fact" (that light can pass undisturbed through air notwithstanding the immense number of air particles it encounters) "can be accounted for if æther is not immensely denser than atmospheric air." The reason for the conclusion is groundless, but it is the conclusion itself that we have here to notice. He adds "with this great density æther possesses also a very great subtlety."

I might also reply to Prof. Bayma, by asking him why we should admit, in order to explain electric and optical phenomena, two substances so distinct as the repulsive envelop of molecules, and the attractive luminiferous ether. The evidence of their similarity is much greater than of their dissimilarity.

In speaking of the two ethers as subtle, it was meant that a large number of their atoms occupied the interstices between the atoms of gross matter. It was also of course recognized that the velocity of propagation of a wave is much greater through either of the ethereal fluids than through a mass of ordinary matter. The only apparent force in the question under consideration, is derived from the fact that a vague conjecture is apt to be raised by it that a single ether may be equal to all the duty now assigned to both.

To proceed with our quotations:

> What we have said on the constitution of molecules demonstrates indeed the necessity of granting to each molecule of ponderable matter a repulsive atmosphere, which we have called the *molecular envelope*. But this envelope is not of æther, since æther is not repulsive.

That is, is not of the same substance as his luminiferous ether, which he regards as attractive. But the "atmosphere" which corresponds in its direct operation with Prof. Bayma's "molecular envelope" is composed of electric matter, and this is repulsive. It is true that I conceive the interstitial spaces of this electric matter, and the space between it and the central atom to be pervaded by the ether of space; but the mechanical part chiefly played by this condensed universal ether consists in its being the medium in which pulses are originated that constitute the force of *heat repulsion*.

Had Professor Norton known the impossibility of continuous matter, he would have found out that what he calls an atom of gross matter comprises already not only the central element of a molecule, but its nuclei and its envelope; and consequently is already endued with the properties and invested with the arrangements which enable it to exert forces of attraction and repulsion upon other molecules; without requiring any new and special atmosphere of electric or luminiferous æther.

That is, in other words, as already shown, Prof. Bayma's nucleus and envelop are in all outward relations precisely correspondent to my central atom and electric atmosphere. The only essential point of difference between us lies in the fact that I conceive that the interstitial luminiferous ether is condensed around the central atom, and is concerned in the production of some of the phenomena. It is not easy to see how Professor Bayma escapes the conclusion that his interstitial ether, which is attracted by the central nucleus, is condensed around it: still it is plain that he imagines that all natural phenomena are produced by the mutual actions of molecules composed of a central nucleus and a single repulsive envelop, without the intervention of any ether; except the luminiferous, in the case of the phenomena of light and radiant heat. This will appear from the following quotation:

As for the examples, by which he illustrates the theory, they consist of a series of phenomena of different kinds, the explanation of which does not show that the theory is not at fault. For it must be remarked, that those explanations do not imply the existence of extended atoms or of two distinct ætherial substances; and therefore the theory assumes more than is necessary for, or guaranteed by, the explanation of phenomena.

When he has shown this to be true of even the ordinary calorific and electric phenomena, we will admit that his objection to a second ethereal atmosphere interpenetrating the first, may have some force. He has given no hint of the general manner in which he supposes electric phenomena to be evolved. Heat he conceives to originate in the vibrations of the molecules of bodies; but, it can be proved almost to a demonstration, that heat cannot originate in this manner.

Our author proceeds as follows:

The atoms of gross matter being "indivisible" cannot be extended, and cannot be conceived to be "spherical in form, for if they were extended and indivisible they would be so many pieces of continuous matter, which we have already proved to be impossible.

To this I have the following replies to offer.

1. Professor Bayma assumes that every point of matter acts instantaneously upon every other point, at all distances however great or small, with a force having the same character at all distances, and inversely proportional to the square of the distance. This may seem probable, but is not self-evident; and in fact no reason can be assigned why one material point, having no extent, should act upon another with a force decreasing with the distance, according to any law whatever. The law of inverse squares is a consequence of wave propagation, or of radiations along definite lines, received on a molecule of definite size, and cannot be predicated of a force that acts instantaneously between two mathematical points. To suppose such a law is an arbitrary assumption.

2. If matter consists of material points, as supposed by Prof. Bayma, it is no more difficult to conceive of an atom of continuous matter, than of the space coextensive with it.

3. It is not more difficult to conceive of an indivisible atom acting as a whole upon another atom, with a certain energy, than of a mere point acting upon another point, and causing it to change its place; at the same time transferring to a new point all the properties it possesses.

4. If the occult nature of the force of action of one material point on another, be such that the intensity becomes indefinitely small at indefinitely small distances, instead of infinitely great as imagined by Prof. Bayma, then a collection of an infinite number of material points may form one invariable atom. Since the size of the atom may, in every instance, be so inappreciable in comparison with the distance between the nearest atoms, that there may never be any inequality of extraneous action on different points of the same atom, imparting different velocities to them, and so tending to break up the continuity of the matter. Besides, we have already seen that no inequality of elementary action, by reason of a difference of distance is legitimately deducible from Prof. Bayma's premises.

5. In speaking of atoms of gross matter as "indivisible," no other ground was intended to be taken than that each atom was indestructible from any possible action of another atom, and essentially invariable in form. This does not preclude the idea that the atom may be an aggregation of a finite number of material points; for it may be that the mutual action of two attractive points passes into a repulsion at excessively minute distances, and so that an atom of ordinary matter may be a system of material points, in either a statical or dynamical equilibrium. Indivisibility, taken in the only

sense in which the term can properly be used, does not then necessarily imply continuity, as maintained by Prof. Bayma.

6. The assumption that each atom is "spherical in form," was adopted merely as the simplest embodiment of the fundamental principles that the action of the atom was equal in all directions, and that the attractive action upon an atom of ether was neutralized at minute distances by the resistance developed at the point of contact. The existence of such a resistance necessarily implies that the elementary parts of the attractive atom, whether finite or infinite in number, act repulsively at very minute distances. But another conception may be formed of the mode of operation of an atom of gross matter, which involves no other supposition than that it acts equally outward in all directions from a center, and takes no account of its geometrical extent. This is, that *the effective attraction of the atom for the ether of space is due to the existence of a repulsion less than would be exerted by the one or more atoms of ether that would naturally occupy its place.* The result would be the condensation of an atmosphere of ether around the atom, without the exertion of any direct attractive force, or of any additional force of resistance. We may conceive the molecular atmosphere of electric ether to originate in a similar way; but as the opportunity of examining and testing this idea sufficiently has not yet been obtained, I shall continue to regard the electric ether as directly attracted by the atom of gross matter, and that the antagonistic force of resistance is furnished by the repulsion of the luminiferous ether condensed around the atom.

If, in accordance with these views, we seek for a possible origin of *gravitation*, we can find it in a primary attraction subsisting between atoms of gross matter. This must be excessively feeble in comparison with "molecular forces," and modify the effect of those forces only by creating a slight additional pressure of contiguous molecular atmospheres. Should we assume the primary actions between atoms of all kinds, to be wholly repulsive, and the effective attraction of the gross atom for both its etherial atmospheres to be a mere consequence of inequalities of repulsion, it is conceivable that the attraction of gravitation might result from ethereal waves, as maintained by Professor Challis;—these waves having their origin in a *dynamical* equilibrium of the atmosphere of universal ether condensed around each atom.

Another critical remark is the following.

Again, æthereal substance according to the author, is repulsive; now this is inconsistent with astronomical facts, as we have sufficiently shown.

The principal astronomical fact, here referred to, is that the planets do not encounter any sensible resistance in their motion through space. The evidence of an ethereal resistance afforded by Encke's comet, Prof. Bayma strives to explain away without success. The fact that no sensible resistance is experienced by the planets does not necessarily imply, as he supposes, that the ether is not repulsive. For, in the first place, if the molecules of the planetary mass have the constitution I have attributed to them, the impinging ether must take effect upon either the ethereal or the electric atmospheres of the molecules, and so may be mostly expended in the generation of heat and electric currents. I have in fact undertaken to show, in my paper on Molecular Physics, that the earth may derive its magnetic condition, and a certain portion of its heat, from the impact of the ether of space. Again, if the action of gravity be not instantaneous it will take effect in a direction slightly inclined to the radius-vector, and, in the existing state of the planetary system, the tangential component resulting from this inclination may be in equilibrium with the feeble overplus of resistance from the ether. Besides, the supposed difficulty is not removed by substituting an attractive for a repulsive ether. It is true that when a molecule of the earth's mass encounters an atom of the ether on the line of its advance, it will, upon Prof. Bayma's ideas, pass through it, and leave it behind; but he has failed to note the fact that during the approach of the two, their relative velocity will be equal to the sum of the velocity of the earth and that due to their mutual attraction, and during their separation will be equal to the difference of the same velocities, and hence that the atom of ether will continue to attract the molecule during a longer interval of time while the two are separating than while they are approaching. The molecule will therefore on the whole, be retarded by the action of the atom. If the attractive ether be "immensely denser than atmospheric air," the resistance should certainly not be less than that of a subtile repulsive ether. If Prof. Bayma should still hold to the same line of argument, I do not see but he must abolish the ether of space altogether.

He continues:

Moreover, the writer, after having assumed that the electric and luminiferous ethers are both made up of atoms that repel each other assumes also that electric æther attracts luminiferous æther; for he admits that a molecule is formed of an atom of gross matter with two atmospheres, of which the first consisting of condensed luminiferous æther is attracted by the other, which consists of electric æther. Now if the atoms of electric æther are repulsive, how can they attract? So then we must conclude that Prof. Nor-

tons's theory as presented by him, in spite of the talent and learning of its author, cannot be adopted in science.

Prof. Bayma has here entirely misunderstood me, and represented what I threw out as a possible and perhaps probable conception, to be a fundamental principle of my theory. The real fundamental principle was that the atoms of electric ether repelled each other, and it was merely conjectured that this repulsion might be due to atmospheres of luminiferous ether condensed around the electric atoms, instead of being a direct repulsive action. It is a little singular that in view of this distinct statement of the manner in which the repulsion might result from a possible attraction, that our author should ask the question, "Now if the atoms of electric ether are repulsive, how can they attract?", and thereupon intimate the existence of a discrepancy fatal to the theory. It is in fact, altogether immaterial whether the mutual repulsion of electric atoms is indirect as conjectured, or direct.

It has now been made sufficiently apparent that the objections urged against my theory of molecular physics have no real force, and that its fundamental principles have not been disturbed. Whether it will ultimately be "adopted in science" or not, must depend upon its availability in rendering a satisfactory account of phenomena, and its ability to withstand the test of a detailed comparison with the entire range of physical facts. If life and health are granted me, I shall endeavor in good time to show, to the satisfaction of every candid mind, that the natural phenomena and experimental results, with their laws and features of diversity, that make up the different departments of Physics, are legitimately deducible from the fundamental principles of the theory; and that it presents claims to acceptance superior to those which can be urged in favor of any other theory.

The attempt to deduce the existing constitution of things and prominent phenomena, by Prof. Bayma from his fundamental ideas, so far as made, has certainly failed at several important points. To specify one or two of these. He obtains a curve of molecular action, that represents a repulsion at the smallest distances succeeded by an attraction at greater distances. This can only be made to represent the three states of bodies by conceiving the molecules of a gas to be in such a condition that if it were entirely freed from pressure, it would expand into a liquid. We know that many gases can be compressed into a liquid, but it is altogether gratuitous to suppose that they could be brought into a similar condition by a diminution of pressure. Experiment has given no indication of such a result or tendency.

Heat and light he conceives to originate in vibrations of gross molecules, but against this notion, as I shall take another occasion to show, insuperable objections may be urged. If this be given up, his explanation of the changes of the state of bodies must also be abandoned.

The doctrine that "transparent bodies transmit rays of light by the motions of their own molecules" will hardly be accepted, we think, by physicists. It would be a waste of time to argue against it.

Again the notion that a certain substance radiates light of a certain color because its molecules are made to vibrate in unison with the ray of that color, will not stand; for the results of spectral analysis show that the parts of a body which are capable by vibration of giving out any color, are precisely those which absorb and stifle that color. This fact, we may add, also proves conclusively that the rays cannot be transmitted by the motion of the molecules. Though so radically at variance with Prof. Bayma's theoretical views, it is in entire accordance with my own, for according to these, light originates in certain vibratory movements of the atoms of the electric atmospheres of molecules, and when these vibrate naturally in unison with the ray of any color that falls upon them, they take up its vis viva, and so the ray is transformed into a molecular electric current.

As to the "leading principles" laid down by the author, they may in the main be conceded; but these by no means cover the whole ground upon which his theory is raised. We find, for example, that he assumes that all elements or material points of the same form of matter act, under similar circumstances, with the same intensity. Now if this principle be admitted, what theoretical basis have we for the existence of distinct primitive molecules for every different substance;—the number of elements associated together being exactly the same for each primitive molecule of each substance, and different for primitive molecules of different substances. The natural tendency would be to a fortuitous association of elements in an endless variety of numbers, into groups. No controlling principle by which uniformity would be evolved from chaotic confusion, is furnished by the theory. The Hand of the Creator must be supposed to have miraculously interfered, and guided each element to its precise place, in the formation of every molecule of matter. The objection here urged, derives still greater force from the consideration that both the nucleus and envelop of each specific molecule are assumed to have a regular geometrical form, different for each substance. To assume the existence of such molecules, is to make an incalculable number of arbitrary

assumptions. No such exception can be taken to the views I have advocated. For primarily each specific atom of gross attractive matter must appropriate to itself from the universally diffused repulsive ethers, its electric and its ethereal atmosphere, each of a certain definite extent. Upon the relations of these specific atmospheres to the central atom and to one another, all the different properties of each specific molecule must depend.

We have already seen that the principle that one material point acts upon another, instantaneously, without the intervention of any medium, is opposed to the fundamental idea that the force exerted is inversely proportional to the square of the distance. This law, to say the least, is an arbitrary assumption in the premises. The author also conceives that the mutual action of two material points, is in no degree and under no circumstances intercepted by another intervening point. But we know that in the case of the molecular forces, the amount of vis viva expended in imparting motion to one particle is abstracted from the force in action, and according to Prof. Bayma, the molecular forces are of the same nature as the forces subsisting between the material elements. The force of gravity, it is true, is not sensibly intercepted, but this does not prove that a tendency to interception does not exist; for, upon the supposition of a wave transmission of the force, the effective attraction of any molecule may be the mere differential of the actual force transmitted, and besides, in the circular revolution of a planet the distance from the sun remains unchanged.

My own doctrine is, that the molecular forces, including the heat-repulsion, are dynamical forces, transmitted by wave-propagation and developed by the primary forces of attraction and repulsion, subsisting between the atoms of gross matter and those of the electric matter and the ether of space. The primary forces determine the electric and ethereal atmospheres of molecules, originate the molecular forces proper, and also when an inequality of electric condition is produced on two contiguous molecules or bodies, by molecular actions, gives rise to the special forces of electric attraction and repulsion. The waves of heat and light originate in the ethereal atmospheres of molecules, and are developed by vibrations of the atoms of the electric atmospheres, toward and from the center of each molecule and the region of ethereal disturbance. By reason of the varying conditions of equilibrium the rate of vibration increases, and its intensity or vis viva, decreases, in proportion as the electric atom is more remote from the center of the molecule. Thus, of the different colored rays, the red proceeds from the lowest depth in the electro-ethereal atmosphere. The obscure heat rays originate at a still lower depth. Heat and light may also

originate in the space between two molecules in the act of combination, or near approach, by reason of the condensation of the interstitial electric ether toward the line of the centers, resulting from the oblique attractive action of the molecules. In this condensation of the electric ether between molecules that are urged nearer to each other, and the expansion of the same when they are separated, we find the key to the explanation of the different modes of electric excitation (that of the galvanic current included). The secret of the intimate relations between electricity and heat and light, is obvious in view of what has been stated.

The ethereal atmospheres of molecules, besides playing the part already signalized, are the chief determining cause of the diverse phenomena that attend the transmission of light through transparent media. Thus *refraction* is chiefly due to the retardation attending the propagation of the ray around from one side to the other of the molecular atmospheres; *dispersion* of the rays in the spectrum, to the fact that the rays of the greatest intensity, and slowest rate of vibration, penetrate to the greatest depth in the molecular atmospheres, pass around in smaller circles, and thus suffer the least retardation; and *double refraction* to the fact that the atmospheres have a spheroidal form, owing to unequal molecular compression on different sides.

Art. XV.—*On Faraday as a Discoverer;* by John Tyndall, F.R.S.*

[Continued from page 51.]

Points of Character.

A point highly illustrative of the character of Faraday now comes into view. He gave an account of his discovery of magneto-electricity in a letter to his friend M. Hachette, of Paris, who communicated the letter to the Academy of Sciences. The letter was translated and published; and immediately afterward two distinguished Italian philosophers took up the subject, made numerous experiments, and published their results before the complete memoirs of Faraday had met the public eye. This evidently irritated him. He reprinted the paper of the learned Italians in the 'Philosophical Magazine,' accompanied by sharp critical notes from himself. He also wrote a letter dated Dec. 1st, 1832, to Gay Lussac, who was then one of the editors of the 'Annales de Chimie,' in which he analyzed the results of the Italian philosophers, pointing out their errors,

* From the Report of the Royal Institution of Great Britain.

and defending himself from what he regarded as imputations on his character. The style of this letter is unexceptionable, for Faraday could not write otherwise than as a gentleman; but the letter shows that had he willed it, he could have hit hard. We have heard much of Faraday's gentleness and sweetness and tenderness. It is all true, but it is very incomplete. You cannot resolve a powerful nature into these elements, and Faraday's character would have been less admirable than it was, had it not embraced forces and tendencies to which the silky adjectives "gentle" and "tender" would by no means apply. Underneath his sweetness and gentleness was the heat of a volcano. He was a man of excitable and fiery nature; but through high self-discipline he had converted the fire into a central glow and motive power of life, instead of permitting it to waste itself in useless passion. "He that is slow to anger," saith the sage, "is greater than the mighty, and he that ruleth his own spirit than he that taketh a city." Faraday was *not* slow to anger, but he completely ruled his own spirit, and thus, though he took no cities, he captivated all hearts.

As already intimated, Faraday had contributed many of his minor papers—including his first analysis of caustic lime—to the 'Quarterly Journal of Science.' In 1832 he collected those papers and others, together in a small octavo volume, labelled them, and prefaced them thus:—

"PAPERS, NOTES, NOTICES, &c., &c.,
published in octavo,
up to 1832.
M. Faraday."

"*Papers* of mine published in octavo, in *Quarterly Journal of Science*, and elsewhere, since the time that Sir H. Davy encouraged me to write the analysis of caustic lime.

"Some, I think, (at this date) are good; others moderate; and some bad. But I have put *all* into the volume, because of the utility they have been of to me,—and none more than the bad,—in pointing out to me in future, or rather, after times, the faults it became me to watch and to avoid.

"As I never looked over one of my papers a year after it was written, without believing both in philosophy and manner it could have been much better done, I still hope the collection may be of great use to me.

"*Aug.* 18, 1832." "M. Faraday.

"None more than the bad!" This is a bit of Faraday's innermost nature; and as I read these words, I am almost constrained to retract what I have said regarding the fire and excitability of his character. But is he not all the more admirable

through his ability to tone down and subdue that fire and that excitability, so as to render himself able to write thus as a little child? I once took the liberty of censuring the conclusion of a letter of his to the Dean of St. Paul's. He subscribed himself "humbly yours," and I objected to the adverb. "Well, but, Tyndall," he said, "I *am* humble; and still it would be a great mistake to think that I am not also proud." This duality ran through his character. A democrat in his defiance of all authority which unfairly limited his freedom of thought, and still ready to stoop in reverence to all that was really worthy of reverence, in the customs of the world or the characters of men.

And here, as well as elsewhere, may be introduced a letter which bears upon this question of self-control, written long years subsequent to the period at which we have now arrived. I had been at Glasgow in 1855, at a meeting of the British Association. On a certain day, I communicated a paper to the physical section, which was followed by a brisk discussion. Men of great distinction took part in it, the late Dr. Whewell among the number, and it waxed warm on both sides. I was by no means content with this discussion; and least of all with my own part in it. This discontent affected me for several days, during which I wrote to Faraday, giving him no details, but expressing in a general way my dissatisfaction. I give the following extract from his reply:—

SYDENHAM, *6th Oct.*, 1855.

"MY DEAR TYNDALL,

"These great meetings, of which I think very well altogether, advance science chiefly by bringing scientific men together, and making them to know and be friends with each other; and I am sorry when that is not the effect in every part of their course. I know nothing except from what you tell me, for I have not yet looked at the reports of the proceedings; but let me, as an old man, who ought by this time to have profited by experience, say that when I was younger, I found I often misinterpreted the intentions of people, and found they did not mean what at the time I supposed they meant; and, further, that as a generul rule, it was better to be a little dull of apprehension, where phrases seemed to imply pique, and quick in perception, when on the contrary they seemed to imply kindly feeling. The real truth never fails ultimately to appear; and opposing parties if wrong, are sooner convinced when replied to forbearingly, than when overwhelmed. All I mean to say is, that it is better to be blind to the results of partisanship, and quick to see good will. One has more happiness in oneself, in endeavoring to follow the things that make for peace. You can hardly imagine how often I have been heated in private when opposed, as I have thought unjustly and superciliously, and yet I have striven, and succeeded I hope, in keeping down replies

of the like kind. And I know I have never lost by it. I would not say all this to you did I not esteem you as a true philosopher and friend.*

"Yours, very truly, M. FARADAY."

Identity of Electricities: First Researches on Electro-Chemistry.

I have already once used the word "discomfort" in reference to the occasional state of Faraday's mind when experimenting. It was to him a discomfort to reason upon data which admitted of doubt. He hated what he called "doubtful knowledge," and ever tended either to transfer it into the region of undoubtful knowledge, or of certain and definite ignorance. Pretence of all kinds, whether in life or philosophy, was hateful to him. He wished to know the reality of our nescience as well as of our science. "Be one thing or the other," he seemed to say to an unproved hypothesis, "come out as a solid truth, or disappear as a convicted lie." After making the great discovery which I have attempted to describe, a doubt seemed to beset him as regards the identity of electricities. "Is it right," he seemed to ask, "to call this agency which I have discovered, electricity at all? Are there perfectly conclusive grounds for believing that the electricity of the machine, the pile, the gymnotus and torpedo, magneto-electricity and thermo-electricity, are merely different manifestations of one and the same agent?" To answer this question to his own satisfaction, he formally reviewed the knowledge of that day. He added to it new experiments of his own, and finally decided in favor of the "Identity of Electricities." His paper upon this subject was read before the Royal Society on the 10th and 17th of January, 1833.

After he had proved to his own satisfaction the identity of electricities, he tried to compare them quantitatively together. The terms quantity and intensity, which Faraday constantly used, need a word of explanation here. He might charge a single Leyden jar by twenty turns of his machine, or he might charge a battery of ten jars by the same number of turns. The *quantity* in both cases would be sensibly the same, but the *intensity* of the single jar would be the greatest, for here the electricity would be less diffused. Faraday first satisfied himself that the needle of his galvanometer was caused to swing through the same arc by the same quantity of machine electricity, whether it was condensed in a small battery or diffused over a

* Faraday would have been rejoiced to learn that, during its last meeting at Dundee, the British Association illustrated in a striking manner the function which he here describes as its principal one. In my own case, a brotherly welcome was everywhere manifested. In fact, the differences of really honorable and sane men are never beyond healing.

large one. Thus the electricity developed by thirty turns of his machine produced, under very variable conditions of battery surface, the same deflection. Hence he inferred the possibility of comparing, as regards quantity, electricities which differ greatly from each other in intensity.

His object now is, to compare frictional with voltaic electricity. Moistening bibulous paper with the iodid of potassium —a favorite test of his—and subjecting it to the action of machine electricity, he decomposed the iodid, and formed a brown spot where the iodid was liberated. Then he immersed two wires, one of zinc, the other of platinum, each $\frac{1}{13}$th of an inch in diameter, to a depth of $\frac{5}{8}$ths of an inch in acidulated water during eight beats of his watch, or $\frac{3}{20}$ths of a second; and found that the needle of his galvanometer swung through the same arc, and colored his moistened paper to the same extent, as thirty turns of his large electrical machine. Twenty-eight turns of the machine produced an effect distinctly less than that produced by his two wires. Now the quantity of water decomposed by the wires in this experiment, totally eluded observation; it was immeasurably small; and still that amount of decomposition involved the development of a quantity of electric force which, if applied in its proper form would kill a rat, and no man would like to bear it.

In his subsequent researches "on the absolute quantity of electricity associated with the particles or atoms of matter," he endeavors to give an idea of the amount of electrical force involved in the decomposition of a single grain of water. He is almost afraid to mention it, for he estimates it at 800,000 discharges of his large Leyden battery. This, if concentrated in a single discharge, would be equal to a very great flash of lightning; while the chemical action of a single grain of water on four grains of zinc would yield electricity equal in quantity to a powerful thunderstorm. Thus his mind rises from the minute to the vast, expanding involuntarily from the smallest laboratory fact till it embraces the largest and grandest of natural phenomena.*

In reality, however, he is at this time only clearing his way, and he continues laboriously to clear it for some time afterward. He is digging the shaft, guided by that instinct toward the

* Buff finds the quantity of electricity associated with one milligram of hydrogen in water, to be equal to 45 480 charges of a Leyden jar, with a height of 480 millimetres, and a diameter of 160 millimetres. Weber and Kohlrausch have calculated that if the quantity of electricity associated with one milligram of hydrogen in water, were diffused over a cloud at a height of 1,000 metres above the earth, it would exert upon an equal quantity of the opposite electricity at the earth's surface an attractive force of 2,268,000 kilograms.—*Electrolytische Maasbestimmungen*, 1866, p. 262.

mineral lode which was to him a rod of divination. "*Er riecht die Wahrheit,*" said the lamented Kohlrausch, an eminent German, once in my hearing:—"He smells the truth." His eyes are now steadily fixed on this wonderful voltaic current, and he must learn more of its mode of transmission.

On the 23d of May, 1833, he read a paper before the Royal Society, "On a new Law of Electric Conduction." He found that though the current passed through water, it did not pass through ice:—why not, since they are one and the same substance? Some years subsequently he answered this question by saying that the liquid condition enables the molecule of water to turn round so as to place itself in the proper line of polarization, while the rigidity of the solid condition prevents this arrangement. This polar arrangement must precede decomposition, and decomposition is an accompaniment of conduction. He then passed on to other substances; to oxdys and chlorids, and iodids, and salts, and sulphurets, and found them all insulators when solid, and conductors when fused. In all cases moreover, except one—and this exception he thought might be apparent only—he found the passage of the current across the fused compound to be accompanied by its decomposition. Is then the act of decomposition essential to the act of conduction in these bodies? Even recently this question was warmly contested. Faraday was very cautious latterly in expressing himself upon this subject; but as a matter of fact he held that an infinitesimal quantity of electricity might pass through a compound liquid without producing its decomposition. De la Rive, who has been a great worker on the chemical phenomena of the pile, is very emphatic on the other side. Experiment, according to him and others, establishes in the most conclusive manner that no trace of electricity can pass through a liquid compound without producing its equivalent decomposition.*

Faraday has now got fairly entangled amid the chemical phenomena of the pile, and here his previous training under Davy must have been of the most important service to him. Why, he asks, should decomposition thus take place? what force is it that wrenches the locked constituent of these compounds asunder? On the 20th of June, 1833, he read a paper before the Royal Society "On Electro-Chemical Decomposition," in which he seeks to answer these questions. The notion has been entertained that the poles as they are called, of the decomposing cell, or in other words the surfaces by which the current enters and quits the liquid, exercised electric attractions upon

* 'Faraday, sa Vie et ses Travaux,' p. 20.

the constituents of the liquid and tore them asunder. Faraday combats this notion with extreme vigor. Litmus reveals, as you know, the action of an acid by turning red, turmeric reveals the action of an alkali by turning brown. Sulphate of soda, you know, is a salt compounded of the alkali soda and sulphuric acid. The voltaic current passing through a solution of this salt so decomposes it, that sulphuric acid appears at one pole of the decomposing cell and alkali at the other. Faraday steeped a piece of litmus paper and a piece of turmeric paper in a solution of sulphate of soda; placing each of them upon a separate plate of glass, he connected them together by means of a string moistened with the same solution. He then attached one of them to the positive conductor of an electric machine, and the other to the gas-pipes of this building. These he called his "discharging train." On turning the machine, the electricity passed from paper to paper through the string, which might be varied in length from a few inches to seventy feet without changing the result. The first paper was reddened, declaring the presence of sulphuric acid; the second was browned, declaring the presence of the alkali soda. The dissolved salt, therefore, arranged in this fashion was decomposed by the machine exactly as it would have been by the voltaic current. When instead of using the positive conductor he used the negative, the positions of the acid and alkali were reversed. Thus he satisfied himself that chemical decomposition by the machine is obedient to the laws which rule decomposition by the pile.

And now he gradually abolishes these so-called poles to the attraction of which electric decomposition had been ascribed. He connected a piece of turmeric paper moistened with the sulphate of soda with the positive conductor of his machine; then he placed a metallic point in connection with his discharging train opposite the moist paper, so that the electricity shall discharge through the air toward the point. The turning of the machine caused the corners of the piece of turmeric paper opposite to the point to turn brown, thus declaring the presence of alkali. He changed the turmeric for litmus paper, and placed it not in connection with his conductor, but with his discharging train, a metallic point connected with the conductor being fixed at a couple of inches from the paper; on turning the machine, acid was liberated at the edges and corners of the litmus. He then placed a series of pointed pieces of paper, each separate piece being composed of two halves, one of litmus and the other of turmeric paper, and all moistened with sulphate of soda, in the line of current from the machine. The pieces of paper were separated from each other by spaces of air. The machine was turned; and it was always

found that at the point where the electricity entered the paper, litmus was reddened, and at the point where it quitted the paper, turmeric was browned. "Here," he urges, "the poles are entirely abandoned, but we have still electro-chemical decomposition." It is evident to him that instead of being *attracted* by the poles, the bodies separated are *ejected* by the current. The effects thus obtained with poles of air he also succeeded in obtaining with poles of water. The advance in Faraday's own ideas made at this time is indicated by the word "ejected." He afterwards reiterates this view: the evolved substances are *expelled* from the decomposing body and "*not drawn out by an attraction.*"

Having abolished this idea of polar attraction, he proceeds to enunciate and develop a theory of his own. He refers to Davy's celebrated Bakerian Lecture given in 1806, which he says "is almost entirely occupied in the consideration of electro-chemical decompositions." The facts recorded in that lecture Faraday regards as of the utmost value. But "the mode of action by which the effects take place is stated very generally; so generally indeed, that probably a dozen precise schemes of electro-chemical action might be drawn up, differing essentially from each other, yet all agreeing with the statement there given."

It appears to me that these words might with justice be applied to Faraday's own researches at this time. They furnish us with results of permanent value; but little help can be found in the theory advanced to account for them. It would, perhaps, be more correct to say that the theory itself is hardly presentable in any tangible form to the intellect. Faraday looks, and rightly looks, into the heart of the decomposing body itself: he sees, and rightly sees, active within it the forces which produce the decomposition, and he rejects, and rightly rejects, the notion of external attraction; but beyond the hypothesis of decompositions and recompositions, enunciated and developed by Grothuss and Davy, he does not, I think, help us to any definite conception as to how the force reaches the decomposing mass and acts within it. Nor, indeed, can this be done, until we know the true physical process which underlies what we call an electric current.

Faraday conceives of that current as "*an axis of power having contrary forces exactly equal in amount in opposite directions:*" but this definition, though much quoted and circulated, teaches us nothing regarding the current. An "axis" here can only mean a direction; and what we want to be able to conceive of is, not the axis along which the power acts, but the nature and mode of action of the power itself. He objects

to the vagueness of De la Rive; but the fact is that both he and De la Rive labor under the same difficulty. Neither wishes to commit himself to the notion of a current compounded of two electricities flowing in two opposite directions; but the time had not come, nor is it yet come, for the displacement of this provisional fiction by the true mechanical conception. Still, however indistinct the theoretic notions of Faraday at this time may be, the facts which are arising before him and around him are leading him gradually, but surely, to results of incalculable importance in relation to the philosophy of the voltaic pile.

He had always some great object of research in view, but in the pursuit of it, he frequently alighted on facts of collateral interest, to examine which he sometimes turned aside from his direct course. Thus we find the series of his researches on electro-chemical decomposition interrupted by an inquiry into "the power of metals and other solids, to induce the combination of gaseous bodies." This inquiry, which was received by the Royal Society on the 30th of November, 1833, though not so important as those which precede and follow it, illustrates throughout his strength as an experimenter. The power of spongy platinum to cause the combination of oxygen and hydrogen had been discovered by Döbereiner in 1823, and had been applied by him in the construction of his well-known philosophic lamp. It was shown subsequently by Dulong and Thenard that even a platinum wire, when perfectly cleansed, may be raised to incandescence by its action on a jet of cold hydrogen.

In his experiments on the decomposition of water, Faraday found that the positive platinum plate of the decomposing cell possessed in an extraordinary degree, the power of causing oxygen and hydrogen to combine. He traced the cause of this to the perfect cleanness of the positive plate. Against it was liberated oxygen, which with the powerful affinity of the "nascent state," swept away all impurity from the surface against which it was liberated. The bubbles of gas liberated on one of the platinum plates or wires of a decomposing cell are always much smaller, and they rise in much more rapid succession than those from the other. Knowing that oxygen is sixteen times heavier than hydrogen, I have more than once concluded, and, I fear, led others into the error of concluding, that the smaller and more quickly rising bubbles must belong to the lighter gas. The thing appeared so obvious that I did not give myself the trouble of looking at the battery, which would at once have told me the nature of the gas. But Faraday would never have

been satisfied with a deduction if he could have reduced it to a fact. And he has taught me that the fact here is the direct reverse of what I supposed it to be. The small bubbles are oxygen, and their smallness is due to the perfect cleanness of the surface on which they are liberated. The hydrogen adhering to the other electrode swells into large bubbles, which rise in much slower succession; but when the current is reversed, the hydrogen is liberated upon the cleansed wire, and then its bubbles also become small.

Laws of Electro-Chemical Decomposition.

In our conceptions and reasonings regarding the forces of nature, we perpetually make use of symbols which, when they possess a high representative value, we dignify with the name of theories. Thus prompted by certain analogies, we ascribe electrical phenomena to the action of a peculiar fluid, sometimes flowing, sometimes at rest. Such conceptions have their advantages and their disadvantages; they afford peaceful lodging to the intellect for a time, but they also circumscribe it, and by and by, when the mind has grown too large for its lodging, it often finds difficulty in breaking down the walls of what has become its prison instead of its home.*

No man ever felt the tyranny of symbols more deeply than Faraday, and no man was ever more assiduous than he to liberate himself from them and the terms which suggested them. Calling Dr. Whewell to his aid in 1833, he endeavored to displace by others all terms tainted by a foregone conclusion. His paper on Electro-chemical decomposition, received by the Royal Society on the 9th of January, 1834, opens with the proposal of a new terminology. He would avoid the word "current" if he could.† He does abandon the word "poles" as applied to the ends of a decomposing cell, because it suggests the idea of attraction, substituting for it the perfectly neutral term *electrodes*. He applied the term *electrolyte* to every substance which can be decomposed by the current, and the act of decomposition, he calls *electrolysis*. All these terms have become current in science. He called the positive electrode the *Anode*, and the negative one the *Cathode*, but these terms, though frequently used, have not enjoyed the same currency as

* I copy these words from the printed abstract of a Friday evening lecture, given by myself, because they remind me of Faraday's voice responding to the utterance by an emphatic *hear! hear!*—*Proceedings of the Royal Institution*, vol. ii, p. 132.

† In 1838 he expresses himself thus:—"The word current is so expressive in common language that when applied in the consideration of electrical phenomena, we can hardly divest it sufficiently of its meaning, or prevent our minds from being prejudiced by it."—*Exp. Researches*, vol. i, p. 515, (§ 1617.)

the others. The terms *Anion* and *Cation*, which he applied to the constituents of the decomposed electrolyte, and the term *ion*, which included both anions and cations, are still less frequently employed.

Faraday now passes from terminology to research; he sees the necessity of quantitive determinations, and seeks to supply himself with a measure of voltaic electricity. This he finds in the quantity of water decomposed by the current. He tests this measure in all possible ways, to assure himself that no error can arise from its employment. He places in the course of one and the same current, a series of cells with electrodes of different sizes, some of them plates of platinum, others merely platinum wires, and collects the gas liberated on each distinct pair of electrodes. He finds the quantity of gas to be the same for all. Thus he concludes that when the same quantity of electricity is caused to pass through a series of cells containing acidulated water, the electro-chemical action is independent of the size of the electrodes. He next proves that variations in intensity do not interfere with this equality of action. Whether his battery is charged with strong acid or with weak; whether it consists of five pairs or fifty pairs; in short, whatever be its source, when the same current is sent through his series of cells, the same amount of decomposition takes place in all. He next assures himself that the strength or weakness of his dilute acid does not interfere with this law. Sending the same current through a series of cells containing mixtures of sulphuric acid and water of different strengths, he finds, however the proportion of acid to water might vary, the same amount of gas to be collected in all the cells. A crowd of facts of this character forced upon Faraday's mind the conclusion that the amount of electro-chemical decomposition depends, not upon the size of the electrodes, not upon the intensity of the current, not upon the strength of the solution, but solely upon the quantity of electricity which passes through the cell. The quantity of electricity, he concludes, is proportional to the amount of chemical action. On this law Faraday based the construction of his celebrated voltameter, or measurer of voltaic electricity.

But before he can apply this measure he must clear his ground of numerous possible sources of error. The decomposition of his acidulated water is certainly a *direct* result of the current; but as the varied and important researches of MM. Becquerel, De la Rive, and others had shown, there are also *secondary* actions, which may materially interfere with and complicate the pure action of the current. These actions may occur in two ways: either the liberated *ion* may seize upon the electrode against which it is set free, forming a chemical compound with

that electrode; or it may seize upon the substance of the electrolyte itself, and thus introduce into the circuit chemical actions over and above those due to the current. Faraday subjected these secondary actions to an exhaustive examination. Instructed by his experiments, and rendered competent by them to distinguish between primary and secondary results, he proceeds to establish the doctrine of "definite electro-chemical decomposition."

Into the same circuit he introduced his voltameter, which consisted of a graduated tube filled with acidulated water and provided with platinum plates for the decomposition of the water, and also a cell containing chlorid of tin. Experiments already referred to had taught him that this substance, though an insulator when solid, is a conductor when fused, the passage of the current being always accompanied by the decomposition of the chlorid. He wished now to ascertain what relation this decomposition bore to that of the water in his voltameter.

Completing his circuit, he permitted the current to continue until "a reasonable quantity of gas" was collected in the voltameter. The circuit was then broken, and the quantity of tin liberated, compared with the quantity of gas. The weight of the former was 3·2 grains, that of the latter 0·49742 of a grain. Oxygen, as you know, unites with hydrogen in the proportion of 8 to 1 to form water. Calling the equivalent, or, as it is sometimes called, the atomic weight of hydrogen 1, that of oxygen is 8; that of water is consequently 8+1, or 9. Now if the quantity of water decomposed in Faraday's experiment be represented by the number 9, or in other words, by the equivalent of water, then the quantity of tin liberated from the fused chlorid is found by an easy calculation to be 57·9, which is almost exactly the chemical equivalent of tin. Thus both the water and the chlorid were broken up in proportions expressed by their respective equivalents. The amount of electric force which wrenched asunder the constituents of the molecule of water was competent, and neither more nor less than competent, to wrench asunder the constituents of the molecules of the chlorid of tin. This fact is typical. With the indications of his voltameter he compared the decomposition of other substances both singly and in series. He submitted his conclusions to numberless tests. He purposely introduced secondary actions. He endeavored to hamper the fulfilment of those laws which it was the intense desire of his mind to see established. But from all these difficulties, emerged the golden truth, that under every variety of circumstances, the decompositions of the voltaic current are as definite in their character as those chemical combinations which gave birth to the atomic theory. This law of

electro-chemical decomposition ranks, in point of importance, with that of definite combining proportions in chemistry.

Origin of Power in the Voltaic Pile.

In one of the public areas of the town of Como stands a statute, with no inscription on its pedestal save that of a single name, "Volta." The bearer of that name occupies a place for ever memorable in the history of science. To him we owe the discovery of the voltaic pile, to which, for a brief interval, we must now turn our attention.

The objects of scientific thought being the passionless laws and phenomena of external nature, one might suppose that their investigation and discussion would be completely withdrawn from the region of the feelings, and pursued by the cold dry light of the intellect alone. This, however, is not always the case. Man carries his heart with him into all his works. You cannot separate the moral and emotional from the intellectual; and thus it is that the discussion of a point of science may rise to the heat of a battle-field. The fight between the rival optical theories of Emission and Undulation was of this fierce character; and scarcely less fierce for many years was the contest as to the origin and maintenance of the power of the voltaic pile. Volta himself supposed it to reside in the contact of different metals. Here was exerted his "electro-motive force," which tore the combined electricities asunder and drove them as currents in opposite directions. To render the circulation of the current possible, it was necessary to connect the metals by a moist conductor; for when any two metals were connected by a third, their relation to each other was such that a complete neutralization of the electric motion was the result. Volta's theory of metallic contact was so clear, so beautiful, and apparently so complete, that the best intellects of Europe accepted it as the expression of natural law.

Volta himself knew nothing of the chemical phenomena of the pile; but as soon as these became known, suggestions and intimations appeared that chemical action, and not metallic contact, might be the real source of voltaic electricity. This idea was expressed by Fabroni in Italy and by Wollaston in England. It was developed and maintained by those "admirable electricians," Becquerel, of Paris, and De la Rive, of Geneva. The contact theory, on the other hand, received its chief development and illustration in Germany. It was long the scientific creed of the great chemists and natural philosophers of that country, and to the present hour there may be some of them unable to liberate themselves from the fascination of their first-love.

After the researches which I have endeavored to place before you, it was impossible for Faraday to avoid taking a side in this controversy. He did so in a paper "On the Electricity of the Voltaic Pile," received by the Royal Society, on the 7th of April, 1834. His position in the controversy might have been predicted. He saw chemical effects going hand-in-hand with electrical effects, the one being proportional to the other; and, in the paper now before us, he proved that when the former were excluded, the latter were sought for in vain. He produced a current without metallic contact; he discovered liquids which, though competent to transmit the feeblest currents—competent therefore to allow the electricity of contact to flow through them if it were able to form a current—were absolutely powerless when chemically inactive.

One of the very few experimental mistakes of Faraday occurred in this investigation. He thought that with a single voltaic cell he had obtained the spark *before the metals touched*, but he subsequently discovered his error. To enable the voltaic spark to pass through air before the terminals of the battery were united, it was necessary to exalt the electro-motive force of the battery by multiplying its elements; but all the elements Faraday possessed were unequal to the task of urging the spark across the shortest measurable space of air. Nor, indeed, could the action of the battery, the different metals of which were in contact with each other, decide the point in question. Still as regards the identity of electricities from various sources, it was at that day of great importance to determine whether or not the voltaic current could jump as a spark across an interval before contact. Faraday's friend, Mr. Gassiot, solved this problem. He erected a battery of 4000 cells, and with it urged a stream of sparks from terminal to terminal, when separated from each other by a measurable space of air.

The memoir on the "Electricity of the Voltaic Pile," published in 1834, appears to have produced but little impression upon the supporters of the contact theory. These indeed were men of too great intellectual weight and insight lightly to take up, or lightly to abandon a theory. Faraday therefore resumed the attack in a paper communicated to the Royal Society, on the 6th of February, 1840. In this paper he hampered his antagonists by a crowd of adverse experiments. He hung difficulty after difficulty about the neck of the contact theory, until in its efforts to escape from his assaults it so changed its character as to become a thing totally different from the theory proposed by Volta. The more persistently it was defended, however, the more clearly did it show itself to be a congeries of devices, bearing the stamp of dialectic skill rather than that of natural truth.

In conclusion, Faraday brought to bear upon it an argument which, had its full weight and purport been understood at the time, would have instantly decided the controversy. "The contact theory," he urged, "assumes that a force which is able to overcome powerful resistance, as for instance that of the conductors, good or bad, through which the current passes, and that again of the electrolytic action where bodies are decomposed by it, *can arise out of nothing:* that without any change in the acting matter, or the consumption of any generating force, a current shall be produced which shall go on forever against a constant resistance, or only be stopped, as in the voltaic trough, by the ruins which its exertion has heaped up in its own course. This would indeed be *a creation of power*, and is like no other force in nature. We have many processes by which the *form* of the power may be so changed, that an apparent *conversion* of one into the other takes place. So we can change chemical force into the electric current, or the current into chemical force. The beautiful experiments of Seebeck and Peltier show the convertibility of heat and electricity; and others by Oersted and myself show the convertibility of electricity and magnetism. *But in no case, not even in those of the Gymnotus and Torpedo, is there a pure creation or a production of power without a corresponding exhaustion of something to supply it.*"

These words were published more than two years before either Mayer printed his brief but celebrated essay on the Forces of Inorganic Nature, or Mr. Joule published his first famous experiments on the Mechanical Value of Heat. They illustrate the fact that before any great scientific principle receives distinct enunciation by individuals, it dwells more or less clearly in the general scientific mind. The intellectual plateau is already high, and our discoverers are those who, like peaks above the plateau, rise a little above the general level of thought at the time.

But many years prior, even to the foregoing utterance of Faraday, a similar argument had been employed. I quote here with equal pleasure and admiration the following passage written by Dr. Roget so far back as 1829. Speaking of the contact theory, he says:—"If there could exist a power having the property ascribed to it by the hypothesis, namely, that of giving continual impulse to a fluid in one constant direction, without being exhausted by its own action, it would differ essentially from all the known powers in nature. All the powers and sources of motion with the operation of which we are acquainted, when producing these peculiar effects, *are expended in the same proportion as those effects are produced; and hence*

arises the impossibility of obtaining by their agency a perpetual effect; or in other words a perpetual motion. But the electro-motive force, ascribed by Volta to the metals when in contact is a force which, as long as a free course is allowed to the electricity it sets in motion, is never expended, and continues to be excited with undiminished power in the production of a never-ceasing effect. Against the truth of such a supposition the probabilities are all but infinite." When this argument, which he employed independently, had clearly fixed itself in his mind, Faraday never cared to experiment further on the source of electricity in the voltaic pile. The argument appeared to him "to remove *the foundation itself* of the contact theory," and he afterwards let it crumble down in peace.*

Researches on Frictional Electricity: Induction: Conduction: Specific Inductive Capacity: Theory of Contiguous Particles.

The burst of power which had filled the four preceding years with an amount of experimental work unparalleled in the history of science partially subsided in 1835, and the only scientific paper contributed by Faraday in that year was a comparatively unimportant one, "On an improved Form of the Voltaic Battery." He brooded for a time: his experiments on electrolysis had long filled his mind; he looked, as already stated, into the very heart of the electrolyte, endeavoring to render the play of its atoms visible to his mental eye. He had no doubt that in this case what is called "the electric current" was propagated from particle to particle of the electrolyte; he accepted the doctrine of decomposition and recomposition which, according to Grothuss and Davy, ran from electrode to electrode. And the thought impressed him more and more that ordinary electric induction was also transmitted and sustained by the action of "*contiguous particles.*"

His first great paper on frictional electricity was sent to the Royal Society on the 30th of November, 1837. We here find him face to face with an idea which beset his mind throughout his whole subsequent life,—the idea of *action at a distance.* It

* To account for the *electric current*, which was really the core of the whole discussion, Faraday demonstrated the impotence of the contact theory as then enunciated and defended. Still, it is certain that two different metals, when brought into contact, charge themselves, the one with positive and the other with negative electricity. I had the pleasure of going over this ground with Kohlrausch in 1849, and his experiments left no doubt upon my mind that the contact electricity of Volta was a reality, though it could produce no current. With one of the beautiful instruments devised by himself, Sir William Thompson has rendered this point capable of sure and easy demonstration; and he and others now hold what may be called *a* contact theory, which, while it takes into account the action of the metals, also embraces the chemical phenomena of the circuit. Helmholtz, I believe, was the first to give the contact theory this new form, in his celebrated essay, *Ueber die Erhaltung der Kraft*, p. 45.

perplexed and bewildered him. In his attempts to get rid of this perplexity he was often unconsciously rebelling against the limitations of the intellect itself. He loved to quote Newton upon this point: over and over again he introduces his memorable words, "That gravity should be innate, inherent, and essential to matter, so that one body may act upon another at a distance through *vacuum* and without the mediation of anything else, by and through which this action and force may be conveyed from one to another, is to me so great an absurdity, that I believe no man who has in philosophical matters a competent faculty of thinking can ever fall into it. Gravity must be caused by an agent acting constantly according to certain laws; but whether this agent be material or immaterial I have left to the consideration of my readers."*

Faraday does not see the same difficulty in his contiguous particles. And yet by transferring the conception from masses to particles we simply lessen size and distance, but we do not alter the quality of the conception. Whatever difficulty the mind experiences in conceiving of action at sensible distances, besets it also when it attempts to conceive of action at insensible distances. Still the investigation of the point whether electric and magnetic effects were wrought out through the intervention of contiguous particles or not, had a physical interest altogether apart from the metaphysical difficulty. Faraday grapples with the subject experimentally. By simple intuition he sees that action at a distance must be exerted in straight lines. Gravity, he knows, will not turn a corner, but exerts its pull along a right line; hence his aim and effort to ascertain whether electric action ever takes place in curved lines. This once proved, it would follow that the action is carried on *by means of a medium* surrounding the electrified bodies. His experiments in 1837, reduced, in his opinion, this point to demonstration. He then found that he could electrify by induction an insulated sphere placed completely in the shadow of a body which screened it from direct action. He pictured the lines of electric force bending round the edges of the screen, and reuniting on the other side of it; and he proved that in many cases the augmentation of the distance between his insulated sphere and the inducing body, instead of lessening, increased the charge of the sphere. This he ascribed to the coalescence of the lines of electric force at some distance behind the screen.

Faraday's theoretic views on this subject have not received general acceptance, but they drove him to experiment, and experiment with him was always prolific of results. By suitable

* Newton's third letter to Bentley.

arrangements he places a metallic sphere in the middle of a large hollow sphere, leaving a space of something more than half an inch between them. The interior sphere was insulated, the external one uninsulated. To the former he communicated a definite charge of electricity. It acted by induction upon the concave surface of the latter, and he examined how this act of induction was affected by placing insulators of various kinds between the two spheres. He tried gases, liquids, and solids, but the solids alone gave him positive results. He constructed two instruments of the foregoing description, equal in size and similar in form. The interior sphere of each communicated with the external air by a brass stem ending in a knob. The apparatus was virtually a Leyden jar, the two coatings of which were the two spheres, with a thick and variable insulator between them. The amount of charge in each jar was determined by bringing a proof-plane into contact with its knob, and measuring by a torsion balance the charge taken away. He first charged one of his instruments, and then dividing the charge with the other, found that when air intervened in both cases, the charge was equally divided. But when shell-lac, sulphur, or spermaceti was interposed between the two spheres of one jar, while air occupies this interval in the other, then he found that the instrument occupied by the "solid dielectric" took *more than half* the original charge. A portion of the charge was absorbed in the dielectric itself. The electricity took time to penetrate the dielectric. Immediately after the discharge of the apparatus no trace of electricity was found upon its knob. But after a time electricity was found there, the charge having gradually returned from the dielectric in which it had been lodged. Different insulators posses this power of permitting the charge to enter them in different degrees. Faraday figured their particles as polarized, and he concluded that the force of induction is propagated from particle to particle of the dielectric from the inner sphere to the outer one. This power of propagation possessed by insulaters he calls their "*Specific Inductive Capacity.*"

Faraday visualizes with the utmost clearness the state of his contiguous particles; one after another they become charged, each succeeding particle depending for its charge upon its predecessor. And now he seeks to break down the wall of partition between conductors and insulators. "Can we not" he says, "by a gradual chain of association carry up discharge from its occurrence in air through spermaceti and water to solutions, and then on to chlorids, oxyds, and metals, without any essential change in its character?" Even copper, he urges, offers a resistance to the transmission of electricity. The action

of its particles differs from those of an insulator only in degree. They are charged like the particles of the insulator, but they discharge with greater ease and rapidity; and this rapidity of molecular discharge is what we call conduction. Conduction then is always preceded by atomic induction; and when through some quality of the body, which Faraday does not define, the atomic discharge is rendered slow and difficult, conduction passes into insulation.

Though they are often obscure, a fine vein of philosophic thought runs through those investigations. The mind of the philosopher dwells amid those agencies which underlie the visible phenomena of Induction and Conduction; and he tries by the strong light of his imagination to see the very molecules of his dielectrics. It would, however, be easy to criticize these researches, easy to show the looseness, and sometimes the inaccuracy, of the phraseology employed; but this critical spirit will get little good out of Faraday. Rather let those who ponder his works seek to realize the object he set before him, not permitting his occasional vagueness to interfere with their appreciation of his speculations. We may see the ripples, and eddies, and vortices of a flowing stream, without being able to resolve all these motions into their constituent elements; and so it sometimes strikes me that Faraday clearly saw the play of fluids and ethers and atoms, though his previous training did not enable him to resolve what he saw into its constituents, or describe it in a manner satisfactory to a mind versed in mechanics. And then again occur, I confess, dark sayings, difficult to be understood, which disturb my confidence in this conclusion. It must, however, always be remembered that he works at the very boundaries of our knowledge, and that his mind habitually dwells in the "boundless contiguity of shade" by which that knowledge is surrounded.

In the researches now under review the ratio of speculation and reasoning to experiment is far higher than in any of Faraday's previous works. Amid much that is entangled and dark we have flashes of wondrous insight and utterances which seem less the product of reasoning than of revelation. I will confine myself here to one example of this divining power:—By his most ingenious device of a rapidly rotating mirror, Wheatstone had proved that electricity required time to pass through a wire, the current reaching the middle of the wire later than its two ends. "If," says Faraday, "the two ends of the wire in Professor Wheatstone's experiments were immediately connected with two large insulated metallic surfaces exposed to the air, so that the primary act of induction, after making the contact for discharge, might be in part removed from the inter-

nal portion of the wire at the first instance, and disposed for the moment on its surface jointly with the air and surrounding conductors, then I venture to anticipate that the middle spark would be more retarded than before. And if those two plates were the inner and outer coatings of a large jar or Leyden battery, then the retardation of the spark would be much greater." This was only a *prediction*, for the experiment was not made.* Sixteen years subsequently, however, the proper conditions came into play, and Faraday was able to show that the observations of Werner Siemens, and Latimer Clark, on subterraneous and submarine wires were illustrations, on a grand scale, of the principle which he had enunciated in 1838. The wires and the surrounding water act as a Leyden jar, and the retardation of the current predicted by Faraday manifests itself in every message sent by such cables.

The meaning of Faraday in these memoirs on Induction and Conduction is, as I have said, by no means always clear ; and the difficulty will be most felt by those who are best trained in ordinary theoretic conceptions. He does not know the reader's needs, and he therefore does not meet them. For instance, he speaks over and over again of the impossibility of charging a body with one electricity, though the impossibility is by no means evident. The key to the difficulty is this. He looks upon every insulated conductor as the inner coating of a Leyden jar. An insulated sphere in the middle of a room is to his mind such a coating ; the walls are the outer coating, while the air between both is the insulator, across which the charge acts by induction. Without this reaction of the walls upon the sphere you could no more, according to Faraday, charge it with electricity, than you could charge a Leyden jar, if its outer coating were removed. Distance with him is immaterial. His strength as a generalizer enables him to dissolve the idea of magnitude ; and if you abolished the walls of the room—even the earth itself—he would make the sun and planets the outer coating of his jar. I dare not contend that Faraday in these memoirs made all his theoretic positions good. But a pure vein of philosophy runs through these writings ; while his experiments and reasonings on the forms and phenomena of electrical discharge are of imperishable importance.

Rest needed—Visit to Switzerland.

The last of these memoirs was dated from the Royal Institution in June, 1838. It concludes the first volume of his

* If Sir Charles Wheatstone could be induced to take up his measurements once more, varying the substances through which, and the conditions under which the current is propagated, he might render great service to science, both theoretic and experimental.

"Experimental Researches on Electricity." In 1840, as already stated, he made his final assault on the contact theory, from which it never recovered.* He was now feeling the effects of the mental strain to which he had been subjected for so many years. During these years he repeatedly broke down. His wife alone witnessed the extent of his prostration, and to her loving care we, and the world, are indebted for the enjoyment of his presence here so long. He found occasional relief in a theatre. He frequently quitted London and went to Brighton and elsewhere, always choosing a situation which commanded a view of the sea, or of some other pleasant horizon, where he could sit and gaze, and feel the gradual revival of the faith that

"Nature never did betray
The heart that loved her!"

But very often, for some days after his removal into the country, he would be unable to do more than sit at a window and look out upon the sea and sky.

In 1841, his state became more serious than it had ever been before. A published letter to Mr. Richard Taylor, dated March 11, 1843, contains an allusion to his previous condition. "You are aware," he says, "that considerations regarding health have prevented me from working or reading on science for the last two years." This, at one period or another of their lives, seems to be the fate of most great investigators. They do not know the limits of their constitutional strength until they have transgressed them. It is, perhaps, right that they should transgress them, in order to ascertain where they lie. Faraday, however, though he went far toward it, did not push his transgression beyond his power of restitution. In 1841, Mrs. Faraday and he went to Switzerland, under the affectionate charge of her brother, Mr. George Barnard, the artist. This time of suffering throws fresh light upon his character. I have said that sweetness and gentleness were not its only constituents; that he was also fiery and strong. At the time now referred to, his fire was low and his strength distilled away; but the residue of his life was neither irritability nor discontent. He was unfit to mingle in society, for conversation was a pain to him; but let us observe the great man-child when alone. He is at the village of Interlachen, enjoying Jungfrau sunsets, and at times watching the Swiss nailers making their nails. He keeps a little journal, in which he describes the process of nail-making, and incidentally throws a luminous beam upon himself.

"Aug. 2d, 1841. Clout nail-making goes on here rather considerably, and is a very neat and pretty operation to observe. I

* See note, p. 195.

love a smith's shop, and anything relating to smithery. *My father was a smith.*

From Interlachen he went to the Falls of the Giessbach, on the pleasant Lake of Brientz. And here we have him watching the shoot of the cataract down its series of precipices. It is shattered into foam at the base of each, and tossed by its own recoil as water-dust through the air. The sun is at his back, shining on the drifting spray, and he thus describes and muses on what he sees:

"August 12th, 1841. To-day every fall was foaming from the abundance of water, and the current of wind brought down by it was in some places too strong to stand against. The sun shone brightly, and the rainbows seen from various points were very beautiful. One at the bottom of a fine but furious fall was very pleasant,—there it remained motionless, whilst the gusts and clouds of spray swept furiously across its place, and were dashed against the rock. It looked like a spirit strong in faith and steadfast in the midst of the storm of passions sweeping across it, and though it might fade and revive, still it held on to the rock, as in hope and giving hope. And the very drops, which in the whirlwind of their fury seemed as if they would carry all away, were made to revive it, and give it greater beauty."

ART. XVII.—*On Enargite from the Morning Star Mine, California;* by EDWARD W. ROOT, Assistant in the Laboratory of the School of Mines, Columbia College, New York.

IN November last I examined a copper ore from the Morning Star Mine, Mogul District, Alpine Co., California, which proved to be Enargite. This mineral occurs in a massive state, and also crystallized in small rhombic prisms, whose planes are much striated. These crystals are of a grayish black color, possess a very brilliant metallic luster and are about a millimeter in length. The massive mineral possesses a somewhat coppery color upon fresh fractures, while the exposed surface has a dark bluish tarnish. It is quite brittle. Streak black. Hardness about 4. Sp. grav. 4·34. B. B. decrepitates quite violently and fuses readily to a globule, giving off arsenical and sulphurous fumes and forming a coating of antimonous acid. With fluxes it gives the copper reactions. In a closed tube it fuses readily, giving off at a gentle heat a yellow sublimate of sulphur, and at an increased heat a reddish sublimate of tersulphid of arsenic. It is insoluble in hydrochloric acid. Soluble in nitric acid with

a residue of sulphur and antimonous acid. Soluble in aqua regia with a separation of sulphur.

Associated with the specimen submitted to analysis, was a little iron pyrites and quartz, while the magnet extracted a few small shining particles, which before the blowpipe gave the reactions for iron and titanic acid.

In the quantitative analysis of the mineral a special portion was taken for the sulphur determination; it was decomposed upon a waterbath with fuming nitric acid, which even in the presence of antimonous acid readily converted the sulphur into sulphuric acid, the solution evaporated to dryness, nitric acid expelled by hydrochloric, and the sulphuric acid precipitated as usual.

Another portion of the mineral was decomposed upon a waterbath with fuming nitric acid, evaporated to dryness, the residue dissolved in hydrochloric acid, the solution boiled with a concentrated solution of sulphurous acid in water, to reduce the arsenic to arsenious acid, and copper, arsenic and antimony precipitated as sulphids by hydrosulphuric acid. The moist sulphids were digested upon a waterbath with sulphid of potassium, which dissolved out the sulphids of arsenic and antimony. The residue, consisting of the sulphid of copper was dissolved in nitric acid, converted into the sulphate, and the copper precipitated in the metallic state by hypophosphite of magnesia, according to Gibbs's method.* The solution containing the arsenic and antimony as double sulphids with sulphid of potassium, was diluted with water, treated with a concentrated solution of sulphurous acid in water to an acid reaction, and boiled in a large glass flask for several hours. Both the sulphids of arsenic and antimony were at first precipitated, but upon boiling, the sulphid of arsenic was converted into arsenious acid. The sulphid of antimony was filtered off, an excess of sulphur removed by repeated treatment with bisulphid of carbon, converted into the antimoniate of antimony (SbO_3 SbO_5) by the action of fuming nitric acid, and estimated as such. The arsenic was precipitated after acidifying the solution with dilute hydrochloric acid, as the sulphid, by hydrochloric acid. The sulphid of arsenic was freed from an excess of sulphur by bisulphid of carbon, converted into arsenic acid by treating with fuming nitric acid upon a waterbath, and precipitated as usual by the magnesia mixture.†

* This Journal, II, xliv, 210.

† Bunsen, the originator of this separation of arsenic and antimony, has lately simplified the estimation of the arsenic, by converting the arsenious acid directly into arsenic acid, by passing a current of chlorine gas through the solution for several hours.

In two analyses the following results were obtained :

	1.	2.	Mean.
S,	31·81	31·51	31·66
Cu,	45·94	45·95	45·95
As,	13·65	13·74	13·70
Sb,	6·03	- - - -	6·03
Fe,	0·81	0·64	0·72
SiO_2,	1·03	1·13	1·08
	99·27		99·14

In analysis No. 2, owing to accident, a part of the antimony was lost, but still some five per cent remained.

If the iron present is considered as iron pyrites, and this together with the silica deducted, the following mean is obtained:

S	Cu	As	Sb		
31·68	47·21	14·06	6·19	=	99·14

These numbers give the ratio 1 : 3 : 8 between the arsenic, copper and sulphur, corresponding to the formula $3Cu_2S + (As, Sb)S_5$, which is that of enargite as described by Breithaupt, Field and Burton.

This California enargite differs however from those heretofore described in having a much larger proportion of the arsenic replaced by antimony. It contains 6 per cent of antimony, while that from Chili analyzed by Field, contained none.* That from Peru analyzed by Plattner, 1·61 per cent.† That from New Granada, analyzed by Taylor, 1·29 per cent,‡ and that from Colorado, analyzed by Burton, 1·37 per cent.‖

School of Mines, March 27, 1868.

ART. XVIII.—*Physical Observations on the Andes and the Amazons;* by JAMES ORTON.

THE following observations were made during a scientific expedition across the continent of South America, in the year 1867. The instruments used were two mercurial barometers (one of them short, beginning to mark at 9,000 feet), a Wollaston boiling-point apparatus, and Boussingault's ground thermometer. They were constructed by Mr. James Green of New York, and furnished by the Smithsonian Institution. The barometers perfectly corresponded ; and on returning to New York, it was found that the long barometer (its companion was broken in the valley of Quito,) after a tour of ten thousand miles had not varied a hair's breadth. The route

* This Journal, II, xxvii, 52.
† Pogg. Ann., lxxx, 383.
‡ Proc. Acad. Sci. Philad., 1857, 168.
‖ This Journal, II, xlv, 34.

was from Guayaquil to Pará via Quito, Rio Napo and the Amazons. The chief value of these observations is derived from the fact that they were made with instruments of precision at many localities whose altitude was hitherto unknown, or obtained by an aneroid* or boiling apparatus. They also test the utility of the thermo-barometer, experiments having been made with it and the standard barometer simultaneously. It is doubtful if the two were ever carried together to such elevations and across the breadth of the continent. Indeed I do not know that any barometer has been taken down the Rio Napo. It was my desire to throw some light upon that strange anomaly in the Amazon valley first revealed by Lieut. Herndon and theorized upon by Lieut. Maury.† All the calculations of altitude are based upon the barometric observations with the Pacific level as a constant. On the Amazons and its tributary no correction is made for monthly variation as it is unknown; the experiments were made in November and December, and the correction would be therefore additive. The heights of points on the river are reduced to the level of the water; while the barometric pressures are given as they were taken, the elevation of the instruments varying from two to sixty feet. Thus at Guayaquil they were twenty feet above the river, at Napo, Coca and Pará, twenty-five feet, and at Pebas, sixty feet. The pressures corresponding to temperatures of boiling water are from Guyot's tables after Regnault revised by Moritz. These are placed along side of the barometric observations (reduced to 32°) in order to show the agreement between the barometer and boiling apparatus. To obtain the mean annual temperatures of important places, the ground thermometer was sunk from two to three feet, and allowed to remain about six hours.

The first desideratum was the level of the Pacific off Ecuador. After many careful calculations I fixed upon 29·930 as the barometric pressure at the freezing point. Herndon assumes 30·000, and his altitudes are therefore too high. I know not Humboldt's base, but his estimates are uniformly high. Those of Boussingault and Visse I have taken second-hand, and am not sure they are correctly copied. Pentland gives 29·944 as the mean barometer off Peru; Duperrey (1823) gives 29·961 at Paita; and Gilliss at sea off Cape Lorenzo, Oct. 2d, found 29·825. Pentland's corrected for latitude would very closely agree with mine.

The site of Quito is very uneven: my altitude is that of

* "A Traveler who carries an aneroid alone with him must not expect accuracy within two or three hundred feet."—Guyot. It is generally higher than the standard in high temperatures.

† See this Journal, vol. xix, p. 385.

the grand Plaza. It is a singular fact that La Condamine (1745), Humboldt (1802), Boussingault (1831), and Visse (1863) give a decreasing altitude. One is tempted to believe that the Andes are sinking. Boussingault contends that this is true of some individual mountains.* The mean of all the estimates given in the table, excepting that of Caldas which is manifestly incorrect, is 9,521. Villavicencio gives 9,485. I quote him simply to call attention to the fact that the estimates in his *Geografia de la Republica del Ecuador* are not reliable. In a balloon ascent made in January, 1864, from Woolwich, Eng., by Glaisher for the British Association, the reading of the barometer at $3^h\ 16^m$ P. M., (corrected and reduced to 32°,) was 20·951 at the estimated height of 9,500 ft. The minimum noticed in Quito was 21·460. The French savans give the length of the seconds pendulum at Quito 3·247753 ft., it being 3·250588 ft. at sea-level on the equator. This is a deduction, not the result of experiment, and gives only 9,166 ft. for the height of Quito. The pendulum experiment at Quito would be very interesting, but great disturbances would doubtless arise from the proximity of so many volcanic mountains.

The observations on Pichincha were taken 80 ft. below the highest pinnacle. That in the crater was made at the foot of the cone of eruption. That on Antisana was taken just above the average snow-limit; and that on Cotopaxi at the base of the cone. Cotopaxi's cone is therefore 6,000 ft. high. At Itulcache begins the series of observations from Quito eastward across the continent. The great Amazonian forest commences about ten miles west of Papallacta. Baeza is situated on a ridge; thence the path to Napo turns from an easterly course directly south through Archidona. Chinipleia was our camp on the Rio Cosanga. The heights on the Amazons as given by other travelers express the altitude above the Atlantic; while I have kept the Pacific as a base. The barometer and boiling point at the Atlantic level are computed; the distance of Pará being taken at 95 miles and the fall of the river two inches to the mile. This makes the Pacific off Ecuador about two feet higher than the Atlantic. Ought we not to expect a difference? Do not the great volcanic mountains on the equator exert an attractive power on the ocean at their feet? At Panamá, the Pacific and Atlantic sink to a common level, for there the Andes drop down to an insignificant altitude. I must add, however, that the obser-

* See Bull. de la Soc. Géol. de France, tom. vi, p. 56, Prof. Schön from observations at Wurtzburg thinks that the atmospheric pressure has increased during the last fifty years. Forbes, 1832.

vations at Guayaquil were taken in July, those at Pará in January with the same instruments. My barometric pressure at Pará may therefore be too high; but the true monthly variation is unknown. At Havana the mean barometer in January is ·200 in *excess* of the yearly mean, and the correction at the equator should be still greater; but Dewey makes the January variation at Pará ·011 *below* the mean. His mean in July is 30·020. We need an extended series of observations at Guayaquil and Pará. If we take Dewey's annual mean at Pará, 29·941 and 35 ft. as the altitude, the barometer at the Atlantic level would stand 29·977. But this makes the Atlantic full 40 ft. below the Pacific. Moreover, if Pará is 35 ft. above the Atlantic, the fall of the river is nearly 4½ inches per mile, which is also absurd. The rate cannot be far from two inches. This would make the fall about fifteen feet and the barometer at the Atlantic 29·932.

Slope and current of the Amazons.

	Distance.	Fall.
Napo village to the Marañon,	600 miles.	21·3 inches per mile.
" " " Pará,	2800 "	6·2 " "
Tabatinga " "	2000 "	2·5 " "

Azevedo and Pinto made the fall from Tabatinga to Pará, 0·9; Castlenau made it a little more. LaCondamine assigns to the Amazon in general a slope of 6·3 in. per mile, which very closely agrees with my calculation. Herndon, who, trusting his boiling apparatus, made Ega over 2,000 ft. high, deduced from this the descent of the Amazons "a little more than a foot per mile, which would about give it a current of 2½ miles per hour!"* He remarks that the current increases considerably after the junction of the Madeira. He calls it 2½ from Pebas to Ega in November, and three below Serpa in February. But the Peruvian navigators consider it from 3 to 3½ at Pebas in December (4 at high water). On the Napo in November I found it 5 below Coca; above Santa Rosa there are rapids. The Brazilian lieutenants make the slope of the Amazons exactly the same between Tabatinga and Manáos as between M. and Pará, or 58 ft. per 1,000 miles. It is interesting to compare the Amazons with other rivers:

Rhone,	fall	24·00 in. per mile;	slope,	1′ 18″
Mississippi,	"	19·87 "	"	1′ 4″
Thames,	"	17·5 "	"	0′ 57″
Nile,	"	6·5 "	"	0′ 21″
Amazons,	"	6·25 "	"	0′ 20″
Ganges,	"	4· "	"	0′ 13″
Ohio,	"	4· "	"	0′ 13″

* Herndon's calculations are very singular. Compare the one on page 258 with one on p. 331.

The Mississippi at St. Louis has nearly the same altitude* as the Amazons at the mouth of the Napo: respective distances from the sea, 1,400 and 2,200 miles. Below St. Louis the fall of the Mississippi is 3 in. per mile.

Accidental variations of the barometer.—These are generally considered as reduced to nothing at the equator. Humboldt says, the regularity of the ebb and flow of the aerial ocean is undisturbed by storms, hurricanes, rain and earthquakes in the torrid zone of the new continent, on the coast and at the elevation of 13,000 ft. At Dodabetta, India, (alt. 8,640 ft.) Lieut. Strachey found that the most violent and variable wind did not affect the range of the barometer. But in the valley of the Amazons strange irregularities have been noticed in every hypsometric instrument, whether mercurial barometer, aneroid or boiling apparatus. No two travelers have noticed the same irregularity; but all (save Azevedo and Pinto), have found a lawless disturbing force. Indeed, the anomaly is so constant, that I am inclined to suspect the record of the Brazilian lieutenants, because it discovers no irregularity. Spix and Martius gave Tabatinga the enormous altitude of 670 ft., and elevated Manáos 556 ft. above the sea. This makes the fall of the first thousand miles of the river five times that of the second. Castlenau found Nauta 365 ft., Pebas, 399 ft., and Manáos 293 ft. above the sea; and complaining that his barometer got out of order, rejected a part of his observations. Herndon, with a boiling apparatus, discovered to his surprise that between Nauta and Ega he was ascending according to the instrument, though by the river and his own senses he was descending. At Nauta water boiled at 211°·3, and at Ega at 208°·2, which would put Nauta about 400 ft. above the Atlantic, and Ega, 800 miles farther down stream, at 2,000 ft.! On the Rio Purus, Chandless' barometer stood higher (29·871) than at Manáos, while the observations of Spruce and Wallace, one with an aneroid, the other with a boiling apparatus, made Manáos *lower* than Pará.

My experience was similar. The barometric pressure was very orderly in its increase from the top of Pichincha down to Pará, excepting on the Napo where at one time both the barometer and boiling apparatus unanimously declared that our canoes were gliding up stream though we were descending at the rate of five miles an hour, and excepting also on the Amazons between Tabatinga and Obidos. The boiling point was more irregular than the height of the mercury. Our rate down the Napo was full 40 miles a day; and the record of the boiling

* Mean bar. at St. Louis, 29·520; at the mouth of the Napo, 29·526.

point for five successive days below the Curaray is as follows: 210°·8 ; 211°·1 ; 211°· ; 210°·9 ; 211°·4.

Similar anomalies have been noticed in other parts of the world. Erman's observations in Siberia would place Jakuzk below the level of the sea of Ockozk ; yet the Lena flows down from Jakuzk to the sea. Von Buch observed that the mean pressure on the shores of the Baltic was less than in France, and imputed the difference to what he calls a *vallé atmospherique*. The barometric mean at Great St. Bernard deviates by 14 ft. from the true height ascertained by leveling ; this Plantamour attributes to an abnormal depression of the temperature of Geneva owing to the neighborhood of the lake. The barometer makes the Caspian Sea 250 ft. higher than the level ; and the Antarctic Ocean, 800 ft. above the Atlantic.

The cause of the strange phenomena in the Amazon valley, Herndon (and after him Maury) ascribed to the pressure of the trade-winds as they are dammed up by the Andes. This gigantic wall of mountains, regarded as the rocky shore of an aerial ocean, is equivalent to 14 in. of the barometric column or $\frac{1}{3}$ the weight of the atmosphere. But this theory is not wholly satisfactory. It is true that the inclination of the continent to the east would naturally give a higher barometer than if it were a level table land. "When the atmosphere is swept *en masse* from the sea up the gradual slope of the land surface, we must expect (says Herschel) great discordances between barometric and trigonometric determinations of altitudes." But if the banking up of the trade-winds is the sole cause of the perturbations, ought we not to expect either a gradual though inordinate increase of the barometric pressure in ascending the Amazons, or fixed points of maximum intensity ; yet the fluctuations seem to follow no rule. Herndon found the greatest disturbance above Tabatinga ; I found it below. Herndon made Ega 2,000 ft. above the sea ; I found it 100. Compare also the various altitudes given to Manáos. It is singular that in Herndon's Meteorological Journal, we find a light breeze, *seldom eastward*, between the Huallaga and Ega, while between Ega and Manáos the prevailing wind is east and stronger. The wind theory would lead us to expect just the reverse. It cannot be said that the difference in the observations is due to the fact that they were taken at different seasons of the year. Herndon and myself were at Ega in the same month (December) ; he boiled water at 208°·2 ; I had to raise it to 211°·9. I was at Pebas just a month later than Herndon ; yet we found the boiling point exactly the same. The trade-winds doubtless have something

to do with these variations, as also the position of the moon.* But may not *one* disturbing force be found in the presence of the great Amazonian forest which is a powerful condenser of aqueous vapor? The vapory winds, fresh from the Atlantic, would give a low barometer; after condensation, the mercury would rise.

Overlooking the irregularities between Tabatinga and Obidos, I think my barometrical observations across the continent faithfully delineate the main features from Guayaquil to Pará. We see a striking difference between South America and our own continent in the fact that while Tabatinga, the half-way station, is not over 255 ft. high, Fort Leavenworth at the same distance from the sea, is six hundred feet above it. I will here notice a curious coincidence (it can be nothing more) in the relation between the eastern profile of a continent and its section. Thus, the eastern coast line of the American continents is a rough copy of the line describing the surface from west to east. In the eastern hemisphere, the eastern contour more nearly approximates a section from north to south; and there is some reason for this, for the ocean would eat most deeply into the land between the mountain chains. In the case of South America, the protuberance of Cape St. Roque represents the swell of the Andes; the slope thence to Patagonia is an imitation of the Amazonian valley; while the upward turn of the Tierra del Fuego reminds us of the low Brazilian mountains.

Diurnal variations of the barometer.—These occur with great regularity, notwithstanding violent winds, sudden changes of temperature and humidity. The configuration of the land in India greatly modify the atmospheric tides; but at the foot of the Andes the variations attain their extreme limits at the same hours (10 A. M. and 4 P. M.) as in the high valleys.† The hour of the day may be determined by the barometer at Quito within 15 minutes. The mean daily amplitude,

At Guayaquil is	·091	(Visse, ·147; Darondeau, ·145.)
" Quito,	·084	(Visse, ·090.)
" Pebas,	·104	
" Pará,	·088	

* Sabine found at St. Helena that the mercury was ·004 in. higher when the moon was crossing the meridian above and below the horizon.

† The daily tides were not observed by Richardson at Gt. Bear Lake, lat. 66° 54′; while Prof. Forbes asserts that the barometer at St. Bernard (8,000 ft. high) is lowest at 9 A. M. and highest at 3 P. M; and that Capt. Parry noticed the same reversion in lat. 74°. Dr. Hayes found the maximum at Port Foulke 6½ P. M. and minimum, 3 A. M.

The extreme range decreases with the latitude and also with the increase of altitude:

At Port Foulke,	lat. 78° 18′ N.	it is	1·81 in.	(Sonntag.)
" Pará,	1° 30′ S.	"	·30 "	(Wallace.)
" Guayaquil,	2° 12′ S.	"	·166 "	(Orton.)
" Quito,	0° 13′ S.	"	·130 "	(Orton; LaCondamine, ·133.)
" Antisana Hacienda,	0° 30′ S.	"	·065 "	(Aguirre.)

The diurnal variation decreases with the increase of altitude as well as with the increase of latitude:

Increase of elevation—

Guayaquil,	alt.	10 ft.	var. ·091	
Pará,	"	15	" ·088	
Quito,	"	9500	" ·084	
Antisana,	"	13,300	" ·022	(Aguirre.)
Anomalous, Pebas,	"	345	" ·104	
" Manáos,	"	120	" ·111	(Spruce.)

Increase of latitude—

Guayaquil,	lat. 2° 12′ S.	var. ·091		
	" 23° 55′ N.	" ·060	(computed by Kaemtz.)	
	" 39° 4′ N.	" ·044	"	" "
Arctic Sea,	" 72° 15′ N.	" ·038	(Sonntag.)	
"	" 78° 28′ N.	" ·013	"	
"	" 81° 30′ N.	" ·001	"	

Increase of altitude with increase of latitude—

Paris,	lat. 48° 50′	alt. 200 ft.	var. ·0315	
Geneva,	" 46° 12′	" 350 "	" ·0354	
Faulhorn,	" 46° 30′	" 9000 "	" ·0106	

Max. fluctuation at Port Foulke is in April; minimum, October.
" " Philadelphia, Jan.; " May.
" " Antisana, June; " December.

Throughout Asia the barometric mean is less in summer than in winter; in Pará, Antisana and Sitka it is the reverse. The monthly variation decreases from the poles to about latitude 40°; thence to the equator it increases. At the equator, (I know not how it is in high latitudes) it decreases with the altitude.

The annual range at Port Foulke (0·4 in.) is about 20 times the diurnal; that of Antisana (0·05) is about twice the diurnal. The annual variation is more decidedly marked in India than in tropical America. This is owing, says Dove, to the comparatively constant wind throughout the year in the New World; while in India, equatorial currents prevail when the sun's altitude is greatest and polar currents when it is least.

Comparison of mercurial barometer with boiling apparatus.—After numerous observations taken with these instruments side by side from the sea-level to the top of Pichincha, I conclude that the boiling apparatus, though very convenient, can not be depended upon and is the least reliable in high altitudes. In the Amazonian valley it is certainly useless. The accuracy of Regnault's Tables of corresponding pressures was tested on the Andes by Visse in 1848, and he found the agreement very satisfactory. But my experiments on the same ground do not confirm this. At Guayaquil he found a difference of ·0092 in.; I found ·008. At Quito his difference was ·015; mine ·045. On Pichincha, his difference was ·0045; mine, taken at the same hour of the day, was ·008. At Papallacta, the boiling point indicated a pressure ·205 too small, and at Archidona a pressure ·364 too great. On Mont Blanc, Saussure found the barometer 17·136, and the pressure indicated by boiling water, 17·883. At Rochester, they differ by ·130.

Mean annual temperature.

Guayaquil, 83° (Boussingault with ground thermometer sunk one foot found 78°·8; Hall with six months obs. with air thermometer found 78°·03. Appleton's Am. Cyclop. gives 88°.)
Quito, 58°·8 (LaCondamine, 58°; Humboldt, 57°·92; Boussingault, with ground ther. sunk one foot, 59°·36; Caldas, 59°·; Hall and Salaza, 59°·6; Aguirre, 58°·1.)
Archidona, 77°·
Santa Rosa, 79°·5
Pebas, 80°· (Castelnau, 79°·7)
Tabatinga, 82°· (" 79°·34)
Pará, 80°·2 (Dewey, 80°·5)

The isothermal line in the United States corresponding to the mean temperature of the valley of Quito runs through the state of Tennessee. The temperature of Nashville is 58°·5. The mean temperature at Fort Massachusetts on the Rocky Mountain plateau (alt. 8400 ft.) is 41°·1. The temperature of Quito is just 1° less than that of Rome; but the spring months at the two cities have the same mean temperature. The mean temperatures of Antisana Hacienda and Quebec are the same, but the seasons are nearly reversed.

	Spring.	Summer.	Autumn.	Winter.	Year.
Quebec,	38°·00	67°·67	43°·67	13°·33	40°·67
Antisana,	42°·16	38°·26	40°·70	41°·80	40°·70

The coldest hour at Quito is 6 A. M.; the warmest between 2 and 3 P. M. The latter corresponds with that on the east coasts of the United States and Asia; on the west coast of America and Europe it is 1 or 1½ P. M. The greatest heat at Pará occurs at 2 P. M.; the hottest month is November and the

coolest is March. The decrement of temperature in ascending from Geneva to St. Bernard is 1° for every 487 ft.; between Quito and Antisana it is 1° for every 340 ft.

Barometrical measurements across South America.

Locality.	Alt.	Barom.	Boiling Point.	Regnault's Equiv.	Difference.	Other estimates.
			°			
Pacific Ocean,	0	29·930	212·01			*Bar.* of Visse, 29·904; Boussingault, 29·867.
Guayaquil,	10	29·899	211·95	29·891	−·008	*B. P.* of Visse, 211°·8.
Guaranda,	8840	21·976				*Alt.* of Visse, 8,872; Hall, 8,928.
Arenal,	14,250	18·123				*Alt.* of Visse, 13,917; Hall, 14,268.
Mocha,	10,900	20·393				
Ambato,	8490	22·241				*Alt.* of Visse, 8,541; Boussingault, 8,787.
						Bar. of Jameson, 22,218.
Tacunga,	9181	21·693				*Alt.* of Visse, 9,180; Boussingault, 9,384.
						Bar. of Jameson, 21·700.
Tiupullo.	11,662	19·858				*Alt.* of Visse, 11,702.
Machachi,	9900	21·212				*Alt.* of Visse, 9,823.
Quito,	9520	21·530	195·8	21·485	−·045	*Alt.* of LaCondamine, 9,596; Humboldt, 9,570; Caldas, 8,947; Boussingault, 9567; Visse, 9307; Aguilar, 9496; Bureau des longs. 9540; Tramblay's *Ann.* 9538; Jameson, 9513. *Bar.* of LaCondamine, 21,404; Humboldt, 21,403; Aguilar, 21·465; Jameson, 21·566. *B. P.* of Tramblay, 184°·18; Visse, 195°·6.
Panecillo,	10,101	21·043				*Alt.* of Humboldt, 10,244; Aguilar, 10,135.
						Bar. of Jameson, 21·207. *B. P.* of Visse, 194°·7.
Pichincha, top.	15,827	17·038	184·5	17·030	−·008	*Alt.* of LaCondamine, 15,606; Humboldt, 15,922; Visse, 16,200; Hall, 15,380; Boussingault, 15,676; Jameson, 15,704. *Bar.* of Visse, 16·942.
Pichincha, crater.	13,300		189·2	18·672		*Alt.* of Visse and Moreno, 13,600.
Antisana H.	13,300	18·583				*Alt.* of Humboldt, 13,465; Boussingault, 13,356.
						Bar. of Jameson, 18·630; Aguirre, 18·573.
On Antisana,	16,000	16·782				
Pinatura,	10,410	20·791				*Alt.* of Boussingault, 10,348.
Padregal,	11,860	19·817				
On Cotopaxi,	12,860	19·004				
Riobamba,	9200	21·705				*Alt.* of Boussingault, 9413; Visse, 9157.
Cajabamba,	10,918	20·512				*Alt.* of LaCondamine, 11,000.
Itulcache,	8885	22·006				
Tablon,	10,516	20·800				
Papallacta,	10,511	20·803	193·8	20·598	−·205	

Locality.	Alt.	Barom.	Boiling Point.	Regnault's Equiv.	Difference.	Other estimates.
Guila,	8622	22·206				
Pachamama,	7920	22·751				
Baeza,	6625	23·793				
Chinipleia,	6200	24·145				
Cochachimbamba,	4252	25·832				
Curi-urcu,	3247	26·746				
Arehidona,	2115	27·816	209·00	28·180	+·364	
Napo,	1450	28·419	209·4	28·407	−·012	
Santa Rosa,	1100	28·814	210·4	28·982	+·168	
Coca,	858	29·022	210·65	29·127	+·105	
M'h of R. Aguarico	586	29·321	211·00	29·331	+·010	
do. R. Curaray,	500	29·408	210·8	29·215	−·193	
do. R. Napo,	385	29·526	211·4	29·566	+·040	*Alt.* at Nauta by Castelnau, 365.
Pebas,	345	29·510	211·1	29·390	−·120	*Alt.* of Herndon, 537. *B. P.* of Herndon, 211°·1.
Loreto,			211 4	29·566		
San Antonio,	256	29·655				
Tabatinga,	255	29·656	211·5	29·625	−·041	*Alt.* of Spix and Martius 670; Azevedo and Pinto, 150; Agassiz, 200.
Tunantins,	138?	29·770				*Alt.* of Azevedo and Pinto, 124.
Ega,	100?	29·813	211·9	29 862	+·049	*Alt.* of Herndon, 2052; Azevedo and Pinto, 120; *B. P.* of Herndon, 208°·2.
Manáos,	199?	29·705				*Alt.* of Herndon, 1475; Castelnau, 293; Spix and Martius, 556; Azevedo and Pinto, 92. *B. P.* of Herndon, 209°·3; Gibbon, 210°·87; Wallace, 212°·5.
Serpa,	158?	29·752				*Alt.* of Azevedo and Pinto, 84.
Obidos,	114	29·802				*Alt.* of Azevedo and Pinto, 58; Agassiz, 45.
Santarem,	107	29·808	211·5	29·625	−·183	*Alt.* of Herndon, 846; Azevedo and Pinto, 50. *B. P.* of Herndon, 210°·5.
Mt. Alegre,	83	29·834				
Gurupá,	38	29·890				*Alt.* of Azevedo and Pinto, 42.
Pará,	15	29·889	211·95	29·891	+·002	*Alt.* of Herndon, 320; Azevedo and Pinto, 35; Dewey, 35. *Bar.* of Herndon, 29·708; Dewey, 29·941; Orton (reduced to level of river) 29·914. *B. P.* of Herndon, 211°·5.
Atlantic Ocean,	−2	29·932	212·16			*Bar.* of Dewey, 29·977.

Rochester, N. Y., July 15, 1868.

ART. XIX.—*Notes on the Caucasus;* by Capt. F. VON KOSCHKULL. (Communication addressed to Prof. J. S. NEWBERRY, and translated by him for this Journal.)

THE interest in science manifested by you, led me to communicate to you some facts concerning the Isthmus of the Caucasus; but not satisfied with a verbal recital, you have requested me to make the same communications in writing. Your cordial hospitality and constant assistance to me, a stranger, compel me to comply with your request even under the unfavorable circumstances in which I am placed, for at present with the exception of the little map of the Caucasus lately published by Petermann, I have neither notes nor books of reference by me to enable me to verify the statements I intend to present to you in this report, and I am therefore obliged to rely entirely upon my memory. You are well aware, moreover, that there are many things easy to be communicated by word of mouth, but difficult to express in writing, especially, when one is compelled to trust to memory alone. For this I beg the indulgence, not only of yourself, but of every one who may read this article, that you will not expect precise accuracy, but accept it as a general description of the Isthmus of the Caucasus, prepared with the sole object of showing my willingness to gratify any wish of those toward whom I entertain feelings of the deepest gratitude.

As I propose to speak also of the geology of the Caucasus, I cannot pass by without notice the name of Mr. Abich, since we owe our increased knowledge of the country under consideration entirely to the labors of this celebrated and indefatigable savant who for some time past has devoted himself to the study of this region.

Any communications that I may make concerning the biology and ethnography of this country must be very brief, partly for want of the necessary notes, and partly because, with reference to these subjects, the Caucasus has not, up to this time, been sufficiently explored, owing to the constant wars between Russia and the mountaineers. But since the termination of hostilities in 1864, a scientific exploration of this region has been conducted by Mr. Raddé, the naturalist, and it is to be hoped that the labors of this young and enterprising *savant* will be as successful as those which he prosecuted in the eastern portion of Siberia.

If in this short description there are points which are not quite clear or exact, I trust it will be attributed only to my defective memory, and I once more ask for indulgence from my readers for the reasons which I have stated above.

The country which separates the Black sea and the sea of Azoff from the Caspian, and at the same time connects the south of Russia with Asia Minor and the northern part of Persia, is generally called the Isthmus of the Caucasus. It lies between the parallels of 39° and 46° north latitude, and between the meridians of 54° and 68° east of Greenwich. This region resembles an oblique parallelogram, of which the longitudinal diagonal runs from northwest to southeast, and is about 1200 kilometers long. The transverse diagonal runs from northeast to southwest and its length is about 625 kilometers. All of this country is traversed by ranges of mountains called the Mountains of the Caucasus, and from them the name has passed to the whole Isthmus.

The internal forces of the earth which elevated these mountain chains were exerted in several directions, forming what must be classified as *principal* and *subordinate* lines of upheaval. The directions of the principal lines are, 1st, from north-northwest to south-southeast. This is the trend of the great chain of the Caucasus of which the extremities form the peninsula of Taman on the north-west and Apscheron on the southeast, the latter extending into the Caspian sea. A line of elevation having the same direction in the southern part of the Isthmus is known as the Little Caucasus.

Second direction, from north-northeast to south-southwest. This line is marked by the chain of mountains which bears the name of Karthlo-Imeritia and connects the principal chain of the Isthmus with the Little Caucasus between 41° 40′ and 42° 40′ north latitude, and 60° 40′ and 61° 40′ east longitude. Axes of elevation having this trend are found in the southeast part of the principal chain of the Caucasus in what is known as the country of Daghestan, and also in the Little Caucasus.

The subordinate lines of elevation are first north and south, as best seen on the plateau north of Mt. Elbruz, forming the watershed between the Caspian and the Black sea. Axes with this trend are also seen in Daghestan and the Little Caucasus. Second, east and west. Ranges having this bearing are seen in the mountains of Abkhazi, Swanethi and Imeritia, the southern ramifications of the Grand Caucasus; also in the chain of Trialethi which extends from the Karthlo-Imeritia mountains eastward to the city of Tiflis; and finally the trend is observable in the inferior ranges of Daghestan and the Little Caucasus.

A knowledge of these bearings is not only indispensable to a correct understanding of the structure of the Caucasus, but enables one to comprehend the relations which this great mountain system bears to those on the east and west of the Isthmus. Following the direction of the principal range of the Caucasus

from the peninsula of Apscheron, a submarine elevation can be traced extending southeasterly. The island of Tschelekan is on this line, and its prolongation forms the axis which connects with the mountains of Central Asia.

On the peninsula of Taman, formed by the northwest extremity of the main chain of the Caucasus, is seen the intersection of two lines of upheaval; one, the principal line, running from southeast to northwest, and the other, that of the mountains of the Crimea, from southwest to northeast. The intersection of these two lines mainly determines the configuration of the Isthmus. The northeast and southwest upheaval has not only united the principal chain with the Little Caucasus but has formed a juncture between the mountains of the Isthmus and those of Persia and Asia Minor.

The points of intersection of the main lines are of considerable altitude, as, for example, Sikara, where the principal range crosses the mountains of Karthlo-Imeritia, and Ararat, at the junction of the Persian mountains and those of the Little Caucasus.

The following table gives the position and altitude of the most important peaks of the Isthmus on a northwest and southeast line.*

	North Latitude.	East Longitude.	Height in feet.
	° ′	° ′	
Oschten,	44	57 40	
Fischt,	43 50	57 40	
Schougousch,	43 45	57 55	
Elbruz,	43 20	60	17,425
Dykh-tau,	43	60 40	16,857
Aday-khokh,	42 45	61 30	14,303
Kion-khokh,	42 55	61 40	10,536
Sikara,	42 35	61 40	11,794
Kazbek,	42 40	62 10	15,524
Teboulos-mtha,	42 35	63 15	13,869
Borbalo,	42 30	63 15	9,547
Dyklos-mtha,	42 30	63 35	12,838
Murou-dagh,	41 45	64 10	10,427
Sary-dagh,	41 50	64 20	11,268
Djulty-dagh,	42	64 30	11,668
Alakhoun-dagh,	41 50	64 40	11,912
Sawalat-dagh,	41 25	64 40	11,439
Schalbouz-dagh,	41 20	65	12,834
Schah-dagh,	41 15	65 15	13,090
Bazarduzi,	41 10	65 20	13,814
Dybrar,	40 55	66 20	6,804

* The longitudes are reckoned from Ferro and the heights (it is presumed) are in Paris feet.—EDITORS.

	North Latitude.	East Longitude.	Height in feet.
In the mountains of the Little Caucasus,			
Alagöz,	40° 35′	61° 25′	12,606
Great Ararat,	39 40	62	15,871
Little Ararat,	39 40	62 5	12,056
Kapoutschi-dagh	39 15	63 45	12,061
The most remarkable passes are, *In the main range northwest of the Oschten mountain,*			
Goitkh,	-----	-----	2,300
Pschekh,	-----	-----	5,100
Between the Mts. Fischt and Schougousch,			
Schetlib,	-----	-----	5,600
Between the Mts. Schougousch and Elbruz,			
Psegaschka,	-----	-----	5,809
Santscharo,	-----	-----	7,500
Maroukh,	-----	-----	10,790
Between the Mts. Kazbek and Teboulos-mtha,			
Goudaour,	-----	-----	7,362
In the mountains of the Little Caucasus,			
Near Lake Goktschai,	40 10	62 41	6,000

Since the predominant trend northwest of the Elbruz is from north-northwest to south-southeast, this portion of the Isthmus range is composed of several distinct chains parallel to each other. Between the Elbruz and the Kazbek, the intervention of a northeast and southwest upheaval causes the regularity to disappear. In Daghestan, southeast of the Teboulos-mtha, there are no regular lines of upheaval; the interior forces of the earth having acted in different directions, and thrown the mountains into irregular and entangled masses. The same confusion is found in the Little Caucasus, though the general altitude of the mountains is less than in Daghestan.

Between the Schougousch and Bazaduz mountains, the main chain of the Caucasus presents an unbroken belt of perpetual snow; nevertheless the loftiest portion of the whole Caucasian range is between the mountains of Elbruz and Teboulos-mtha, and here are found the most remarkable glaciers of the Isthmus. Toward the peninsulas of Taman and Apscheron, the altitudes fall off to within a few hundred feet above the level of the sea. In the Little Caucasus there are also snow-covered summits, but they are scattered about and do not present an unbroken belt of snow like those of the principal range.

The formation of the valleys is determined altogether by the arrangement of the mountains. West of the Teboulos-mtha

mountain there are longitudinal valleys, the most important of which are between the Elbruz and the Kazbek; the two valleys of Swanethi on the upper parts of the rivers Ingur and Tzkhlenitz-Tskhale, the valley of Radscha on the Rion, and generally those inhabited by the Ossethes in the midst of the main chain. In Daghestan and the Little Caucasus, there is no regularity in their form or distribution.

The principal range is bounded on the north and south by vast plains running parallel to the mountains, that is, north-northwest and south-southeast. The northern plain merges insensibly into the southern prairies of Russia, the southern serve to separate the two Isthmus ranges.

By the north and south and north-northeast and south-southwest upheavals they are almost equally partitioned. The plateau north of the Elbruz forms, on the northern plain, an eastern division, along the valley of the river Terek, which belongs to the basin of the Caspian sea, and a western division belonging to the Black sea.

The southern plain is divided by the Karthlo-Imeritia mountains into the valley of the Kur, in the Caspian basin, and into that of the river Rion, in the Black sea basin.

These two plains may be considered as the remains of straits which united the Black sea and the sea of Azoff to the Caspian. The upheaval of the Karthlo-Imeritia mountains and the plateau north of the Elbruz mainly caused the separation of these two basins which now show a very considerable difference of level; for the surface of the Caspian is nearly 80 feet below the level of the Black sea, and the level of the latter must be looked for, some distance from the mouths of the rivers Terek and Kouban.

The sedimentary formations of the Isthmus of the Caucasus belong to the Jurassic, Cretaceous and Tertiary periods. With the exception of certain Tertiary beds, none of these strata are rich in fossils, but a sufficient number have been found to establish the fact that we here have representatives of the European deposits indicated below. The Jurassic of the Caucasus corresponds to that of Germany, and, as a consequence, includes the equivalents of the Oolite and Lias, both of which are largely developed. The Wealden is wanting; the Cretaceous strata belong to the epochs of the Neocomian, the greensand or gault and white chalk. The Tertiary strata may be referred to the Eocene, Miocene and Pliocene epochs, all of which are rich in fossils. Among the igneous and metamorphic rocks are granite, gneiss, feldspathic porphyry, diabase, melaphyre, basalt, diorite, trachyte, trachytic tufa, obsidian and phonolite. To these should be added a group of metamorphic schists, viz., chloritic, talcose, and argillaceous. These latter are most abundant in

the central part of the principal chain where they form the serrated crest. The same rocks are also found in the Little Caucasus, but not in great force. From the want of fossils, it is impossible to say whether these metamorphic slates belong to the Liassic formation or one still more ancient.

Granite and gneiss are found upon the northern slope of the principal chain between Elbruz and Kazbek, in the valleys of the tributaries of the Terek. Over all this region the granite occupies the lowest place of all the rocks visible, and, judging from the observations already made, it would seem that the granite masses in the region indicated the base of the principal mountain chain, associated with which are pyroxenic and trachytic rocks, which have been the active agents in the elevation of the metamorphic rocks.

On the southern slope, granite appears at one point in the chain of Karthlo-Imeritia, and here also occupies the lowest position.

The pyroxenic rocks, as melaphyre, pyroxenite, porphyry, basalt, and all the varieties of feldspathic porphyry, make their appearance in the different parts of the principal chain of the Caucasus. These rocks are not only the agents of elevation of the main chain, but they also form the most elevated summits between Fischt and Teboulos-mtha.

At the northwest of Fischt, in Daghestan, all the igneous rocks are covered by sedimentary deposits of enormous thickness, and such as have suffered great changes. In the southeast portion of the Little Caucasus, known under the name of the mountains of Karabagh, amphibolic rocks, diorite and syenite, are found, but the principal agents in the formation of the Little Caucasus would seem to have been trachytic rocks, such as trachytic, trachyte tufa, obsidian, and phonolite. In the western part of this chain, the rock and strata form great plateaus, which are sometimes deeply cut by fissures, in which the draining streams flow. There are also here many crater-form depressions, which are now occupied by water, forming a series of lakes. The most remarkable of the plateaus are those of Gori and Alexandropol, which have an altitude of 4500 feet above the level of the sea, and the plateaus of Akhalkalaki and Bajazette, which are 5000 to 6000 feet above the sea level.

The largest of the lakes to which I have referred are Toporavan, at an elevation of 5600, and Goktschai, 5925 feet. The latter is 70 kilometers long, and 35 kilometers wide.

In the Little Caucasus the trachytic rocks have not only contributed to form the plateaus, but compose many of the conical summits, as Mt. Ararat and Mt. Alagöz. The trachytes are far less abundant in the main chain of the Caucasus, being there limited to the axis of elevation on which Mt. Elbruz is situated.

The sedimentary strata which covers the igneous and crystalline rocks, and which enter so largely into the composition of the mountain chains of the Caucasian isthmus, belong to the Jurassic and Cretaceous formation.

The Tertiary strata are spread over the plains bordering the Great Caucasian chain, both on the north and south, the plains traversed by the rivers Terek and Kuban on the north, the Rion, Kur and Araxes on the south.

From the northwestern extremity of the main chain of the Caucasus, as far east as Mt. Elbruz, the portion of the stratified rocks is quite regular. They here lie in a series of parallel folds, which have a northeasterly trend. It results from the regularity in the position of the strata, that on going from the main chain toward the north, each new element in the geological series represents a more recent, while on going south from the axis, more and more ancient strata are successively passed over.

South and east of Mt. Elbruz, the relations of the different strata become very complicated and obscure, from the intersection of various lines of elevation. In Daghestan and the Little Caucasus, this irregularity in the position of the sedimentary rocks is particularly marked.

In the Tertiary deposits, the Eocene and Miocene beds exhibit some of the undulations which characterize the underlying rocks; but the Pliocene and Arabo-Caspian deposits are almost always quite horizontal.

The line of contact of the amphibolic and pyroxenic rocks, with the other igneous rocks, as well as with the sedimentary strata, is almost always, throughout the Caucasus, marked by the presence of metallic ores. As regards the number of mineral deposits known and worked, the Little Caucasus should be given the first place.

Argentiferous galena has been found in nearly all the mountainous portions of the Caucasian Isthmus. The inhabitants of these districts, warlike in character and devoted to the chase, have always needed lead with which to form their bullets, and from this cause the veins of this mineral have not only been sought and discovered, but in many places they have been extensively worked. To obtain metallic lead from the ore, this latter is melted in small furnaces, in a very rude way, the silver which it contains being neglected.

The most important veins of lead are found on the northern slope of the main chain of the Caucasus, between Mt. Elbruz and Mt. Kazbek, in the valleys of the rivers Ard and Baksan, tributaries of the Terek. Here the galena is accompanied by blende, copper and iron pyrites, in veins cutting granite, diorite, and metamorphic slates.

The mountaineers have had knowledge of these veins for a very long time, and have worked them somewhat, but in a very primitive manner. Ten years since, these deposits were purchased by the Russian government, for the purpose of introducing their improved methods of mining and reduction. After some years spent in explorations, the mines were regularly opened, and smelting works established, which have, up to the present time, produced about 1600 kilograms of silver, and 1,600,000 kilograms of lead. In the Little Caucasus there are veins of argentiferous galena, which were worked in ancient times by the Greeks of Asia Minor, who were able not only to obtain the lead, but also to separate the silver from it. Since the commencement of the present century, these mines have been entirely abandoned by the Greeks, probably from the impossibility of following the veins to great depths, with their imperfect system of mining.

Copper is also found in the Great Caucasus, but in much the larger quantity in the mountains of the Little Caucasus. For the most part, the ores of copper found there are sulphids which occur in veins in erupted masses of diorite. Copper has been mined on the Caucasus from the remotest antiquity. The process employed, both of mining and heating the ores, were of the simplest character, and such as belong to the infancy of the art. The productions of the copper-bearing districts is doubtless due to the richness and abundance of the ore. The average richness is from 10 per cent to 15 per cent metallic copper. The annual production of copper is at present about 156,000 kilograms. In the mines of copper, ores of cobalt have been found within the past two years. These are not treated at home, but are exported to Saxony in large quantities.

The ores of Iron, as specular ore, are found on the southern slope of the principal chain of the Caucasus, on the upper part of the valley of the river Rion, and also in great quantity on the northern slope of the Little Caucasus. Generally these ores are found in veins which accompany erupted masses of porphyry. They have been worked for ages in these two localities, but in a very rude manner. The iron is produced in small Catalan forges, and in blooms that are subsequently farther worked in a kind of blacksmith's forge. Naturally, the production of iron by this process would not suffice for the supply of the country; and the wants of the indispensable metal is mainly met by importations from the furnaces of the Urals. Only within the last five years, have properly built and efficient iron works been established in the country under consideration.

[To be continued.]

ART. XX.—*On some points in the Geology of Vermont;* by T. STERRY HUNT, F.R.S., of the Geological Survey of Canada.

(Read before the meeting of the American Association at Chicago, August, 1868.)

IT is proposed in the present communication, to state some facts with regard to the distribution of the paleozoic rocks of western Vermont, but before doing so it will be well to recall certain points in the geology of the province of Quebec. In this province, to the southeast of the St. Lawrence, there is displayed a great series of rocks to which Sir William Logan has given the name of the Quebec group. In the geology of Canada, published in 1863, this group was divided into two parts, an upper called the Sillery, and a lower the Levis formation, while some black limestones and shales occurring at the base of the latter were referred, doubtfully, to the Potsdam formation. These have, however, since been found to be paleontologically connected with the lower and fossiliferous parts of the Levis formation, as seen in the section on the island of Orleans near Quebec, the whole forming a natural division, which includes 4680 feet at Phillipsburg with the addition of the lower 1285 feet of the Orleans section, where the inferior measures of the group, seen at Phillipsburg, are not brought into view. To this portion of the Quebec group it has been found convenient to restrict the name of the Levis formation, while the succeeding part of what was at first called by that name, extending upward to the base of the Sillery, is distinguished as the Lauzon formation.

The entire thickness of the Levis, so far as known, is over 6,000 feet, and that of the Sillery about 2,000, while the Lauzon is extremely variable, ranging from a few hundred to more than 2,000 feet in thickness.

This middle division is characterized by two bands of magnesian and metalliferous rocks, one near its base, and the other at its summit. The latter band, for convenience of definition, has been united with the succeeding division or formation, and is represented on the geological map as forming the base of the Sillery. These two magnesian bands are of considerable economic importance, and contain interstratified deposits of copper, iron and chrome ores, together with dolomite, magnesite, steatite, serpentine, diorite, chloritic and epidotic rocks, associated with reddish and greenish shales and sandstones. The fauna of the upper divisions of the Quebec group is as yet limited to an *Obolella* and two species of *Lingula* in the Sillery, but the

Levis division has yielded a great number of organic forms which have been carefully studied by Mr. Billings and Prof. James Hall. The latter has described not less than fifty-one species of Graptolitideæ, which are figured in the second decade of Canadian Fossils, while most of the other species, including twenty-eight brachiopods, forty-two gasteropods, twenty cephalopods, and seventy-four crustaceans, have been described by Mr. Billings. The whole number of species described up to this time from the Levis formation, is 219; of these five have been detected in the Chazy and ten in the Calciferous, all the others being peculiar to the Quebec group, whose position is thus, it would seem, clearly defined as intermediate between the Calciferous and Chazy formations; none of its numerous species having been recognized in higher or lower rocks than these. The chief localities of these fossils are at Point Levis and Orleans island near the city of Quebec, and at Phillipsburg and Farnham near Missisquoi bay; but many of the characteristic forms are met with at numerous points between these two regions, the graptolites of the division in particular having been recognized among the cupriferous diorites on the St. Francis at Drummondville, and in the plumbaginous slates near the outlet of Lake Memphramagog.

This group of rocks appears in the province of Quebec along the southeast side of a great fault which has been traced from far below the City of Quebec to the line of Vermont on Missisquoi Bay. By this dislocation it is raised up along side of the rocks of the Trenton and Hudson river formations, and in one place is brought in contact with the Medina formation.

These higher strata, which are in some cases inverted along the line of fault, are often overlaid, and thus made to pass beneath the Quebec group. A little to the south of the province line this same fault brings up a still lower formation, the Red Sandrock of Vermont. This has been traced continuously from near Missisquoi bay to Shoreham, along the east side of the line of fault, while to the west appear the overturned and overlaid strata of the various formations from the Calciferous to the Hudson river formation inclusive. The line of fault is somewhat irregular, and where it has been affected by subsequent denudation, evidences of great overlapping are seen, the strata of the Hudson River formation, according to Sir William Logan, passing in some cases not less than a mile beneath the nearly horizontal layers of the Red Sandrock, which were by the earlier observers very naturally referred to the Medina formation, itself a red sandstone, and next in natural succession to the Hudson River formation in the New York series. In like manner the strata of the Quebec group, which, in Can-

ada, were found overlapping the Trenton limestone were regarded as belonging to the Hudson River formation until the study of their fossils led the way to a knowledge of their true age and relations.

The late Dr. Emmons supposed the Red Sandrock to include the Potsdam and Calciferous formations, though he at the same time completely misunderstood, as will be shown, its stratigraphical relations. Certain trilobites found in this formation by Prof. Adams in 1847 were recognized by Prof. Hall as belonging to the European genus *Conocephalus*, whose geological horizon was then undetermined, but which was afterwards shown to be a primordial type, and led Mr. Billings in 1861, to refer this sandrock to the Potsdam formation. (This Journal, II, xxxii, 232.) Subsequently the Rev. Zadock Thompson discovered in the slates of Georgia the trilobites which were by Emmons described as *Paradoxides*, but by Prof. Hall were recognized as a new genus *Olenellus*, of which he described two species, *O. Vermontana* and *O. Thompsoni*. Notwithstanding its designation, the Red Sandrock of Vermont consists in great part of a red or mottled granular dolomite associated with beds of fucoidal sandstone, conglomerate and slates.

This formation is well displayed in a section examined by Sir William Logan in Swanton, about two miles south of the boundary line between Quebec and Vermont, and described in the Geology of Canada, page 281. The thickness of the series here regarded as belonging to the Potsdam formation is 2,200 feet, intercalated in which, and about 500 feet from the base, occur 130 feet of gray and bluish-black slates, containing the two species of *Olenellus* noticed above, with *Conocephalites*, *Obolella*, *Orthisina*, &c. These slates have been traced southward to a locality in Georgia, where the same species of *Olenellus* occur in similar slates. The strata of the Potsdam formation in Swanton pass to the eastward beneath black shales and thin-bedded limestones holding the characteristic fossils of the Quebec group, and apparently representing the upper portion of the Levis formation. The superposition of these is well seen at Herrick's Mills, besides other places. The Levis formation may be traced northward around the extremity of the Potsdam rocks till it appears between these on the east, and the strata of the Trenton group on the west, forming a tongue which becomes narrower and disappears to the southward where the Potsdam comes directly up against the Trenton group. A little above this point, however, a section from Highgate Springs eastward takes us from the Trenton across a fault bringing up the Levis formation, followed at the distance of a mile farther eastward by a second dislocation by which the Potsdam is

brought up against the Levis, and again passes beneath it to the east, as in the Swanton section. By the second upthrow, a tongue of the Potsdam is carried northward a distance of about ten miles in the midst of the Quebec group. The details of this structure are fully given in the map and sections of this region by Sir William Logan, contained in the atlas accompanying the Geology of Canada. It is to be remarked that the lowermost beds of the Levis formation which are seen to the westward between the Trenton and the Potsdam, do not appear along the eastern limit of the latter, which is overlaid by beds belonging to the summit of the Levis, from which Sir William Logan conjectures a want of conformity between the two formations. The unconformability, in this vicinity, of the overlying rocks with the Potsdam is also to be inferred from the entire absence of the Calciferous from its place between the Potsdam and Levis formations in this locality, although in Newfoundland it appears in its proper position, and with a thickness of no less than 2,700 feet, resting upon more than 5,000 feet of rocks which are referred to the Potsdam group, and overlaid by the Quebec group, which there appears fully developed.

It happens then from the facts already set forth, that the Potsdam formation, which at its outcrop at the foot of the Adirondacks and Laurentides, includes only from 300 to 700 feet of sandstone, is represented a few miles to the eastward by not less than 2,000 feet of dolomites, sandstones and slates, and moreover that occupying a position between the Calciferous and Chazy formations, which are contiguous at their eastern outcrop, there becomes intercalated within this short distance, a fossiliferous group, several thousand feet in thickness. Sir William Logan explains these remarkable differences between the two regions in the following manner. At the beginning of the Silurian period, when the nucleus of the present continent was composed of Laurentian and Huronian rocks, partly rising into hills and in part forming a region of slight elevation, there were deposited in the surrounding seas the first fossiliferous beds of the period. To this succeeded a depression of the bed of the continent, upon the surface of which were then successively laid the shallow water deposits, which are known in New York as the Potsdam sandstone and the Calciferous Sandrock. Meanwhile strata characterized by a similar fauna to these, but having very different lithological character, were formed in the deeper waters which surrounded the continental plateau, this being submerged and elevated at intervals, became overlaid with beds which represent only in a partial and uninterrupted manner, the great succession of strata that were being

accumulated in the adjacent ocean, and which constitute to the eastward, the rocks of Quebec and Vermont, and in the west the great thickness of sediment contemporaneous with those which form the metalliferous series of Lake Superior.

Already, at this early period, movements were going on in the earth's crust, folding and dislocating the paleozoic sediments, within what we may call the oceanic area, while those of the continental plateau were at the same time left undisturbed. This Sir William Logan explains by the resistance offered by the solid continental gneiss. He says, "we may suppose that if a sufficient lateral pressure were applied to strata thus accumulated, and arranged, there would result a series of folds running in a direction at right angles to that of the force, with prevailing overturn dips toward the line of resistance. The solid crystalline gneiss offering more resistance than the newer strata, there resulted a break coinciding with the inclined plane at the junction of these with the gneiss. The lower paleozoic strata pushed up this slope would then raise and fracture the formations above, and be ultimately made to overlap the portion of those resting on the edge of the higher terrace, after probably thrusting over to an inverted dip, the broken edge of the upper formations. The shallow-water strata of the higher terrace relieved from the pressure by the break, would remain comparatively undisturbed, and thus the limit of the more corrugated area would correspond with the slope between the deep and shallow waters of the Potsdam period. The resistance offered by the buttress of gneiss would not only modify the main disturbance, but would probably also guide or modify in some degree, the whole series of parallel corrugations, and thus act as one of the causes giving a direction to the great Appalachian chain of mountains." (*Geol. of Canada*, p. 297.)

In further illustration of this view, I have to communicate a recent discovery which shows the progress of the change from the continental to the oceanic area, and demonstrates that at a point where the Potsdam had already assumed the character of the Red Sandrock, it was overlaid by all of the members of the New York series, up to the Hudson River group, inclusive. It is now some years since Mr. Billings of the Canada Geological Survey received from Messrs. C. H. Hitchcock and A. D. Hager, fossils apparently newer than the Quebec group, from a place to the east of the Potsdam in Sudbury, Vermont, and in 1866, the Rev. Augustus Wing, of Bridport in that state, correctly determined these to belong to the Trenton formation, and sent specimens of the fossils to Mr. Billings, who in April last, visited, and carefully examined the locality, with Mr. James Richardson. The following are the facts observed along a line

of section passing from Crown Point in New York across the head of Lake Champlain and through Bridport to Cornwall in Vermont. Passing from the Laurentian across the various members of the New York series, we reach the limestones of the Trenton group of the east shore of the lake and then encounter the Chazy formation which is brought up along a gentle anticlinal, and is succeeded by the Trenton, Utica, and Hudson River formation all sloping gently to the eastward until we come to the great fault which brings up the Red Sandrock. All the preceding details are shown on Mr. Hitchcock's map which accompanies the Geology of Vermont. This red sandrock which is here seen to overlap the Hudson River formation dips eastward at a small angle and is overlaid by a great mass of limestone holding the characteristic fossils of the Calciferous, among which, as determined by Mr. Billings, are *Ophileta complanata, Bathyurus conicus, Asaphus canalis,* and others too obscure to be identified. These fossils were first discovered by the Rev. Mr. Wing in Sudbury. At the summit of the formation, Mr. Billings was enabled to detect other obscure forms which resemble those of the Levis formation, and seem to indicate the first appearance of the Quebec group in its place between the Calciferous and Chazy formations. Next in ascending sequence, occurs a mass of limestones not less than 2,000 feet in thickness, and probably representing both the Chazy and the Trenton. From it the following Trenton species were obtained by Mr. Billings in Sudbury: *Stenopora fibrosa, Petraia corniculum, Glyptocrinus ramulosus, Ptilodictya acuta, Leptæna sericea, Strophomena alternata, Orthis testudinaria, O. pectinella, Pleurotomaria lenticularis, P. rotuloides* and *Trinucleus concentricus.*

To the east these limestones are cut off by a second dislocation, which brings up the Levis formation, with its characteristic fossils against the Trenton. A ravine marks the line of fault, which has been followed for a distance of more than twenty miles from the southern line of Sudbury northward to Weybridge, where it joins the great western fault. To the west of this line, fossils of the various divisions of the Trenton group are found in numerous localities, and to the east, the characteristic forms of the Levis formation, among which Mr. Billings has detected *Pleurotomaria Quebecensis, P. Missisquoi, Murchisonia Vesta, Ophileta bella, Maclurea matutina, M. pondersa,* and *Bathyurus Saffordi,* besides which many large Orthoceratites are seen in section. These fossils are found in many localities in Sudbury, Cornwall, Middlebury, and Brookville. The limestones are for the most part bluish, but are closely associated with the white marbles quarried in these

localities. Thus at Brookville, Mr. Billings found Levis fossils immediately above and below the white marbles, clearly showing these beds of white crystalline limestone to belong to the lower portion of the Quebec group. This confirms the view expressed by me in this Journal for May, 1861, page 392, that these marbles represent a great development of calcareous matter in the Quebec group and are to be regarded as beds of chemically precipitated carbonate of lime, and not limestones of organic origin. Their great purity and crystalline texture depend upon original conditions of deposition, and not upon any subsequent alteration.

In this connection it may not be out of place to notice some of the views of the late Dr. Emmons which have lately been resuscitated, with certain modifications, by the Rev. J. B. Perry, in a communication to the Boston Society of Natural History in December last. Dr. Emmons conceived the Red Sandrock to be the base of the Silurian system, and to rest upon an older and unconformable series of rocks, to which he gave the name of the Taconic system. His views, though rejected by all American geologists, have been supported by Mr. Jules Marcou, who, however, differs from Dr. Emmons in including the Red Sandrock in the Taconic system, in which he is followed by Mr. Perry. For an exposition of the views of Dr. Emmons and of Mr. Marcou's curious misconception of them, the reader is referred to this Journal, II, xxxii, 427, and xxxiii, 135, 281.

According to Mr. Perry, the slaty rocks which appear to the east and west of the Red Sandrock, belong to two older formations or divisions, which he describes as Lower and Middle Taconic. In the latter he includes the Georgia slate with its primordial species and the Swanton slate considered to be still lower, with graptolites and *Atops trilineatus* Emmons, while he refers to the Lower Taconic, the slates, limestones and conglomerates of the Quebec group. This Middle Taconic is made up of three distinct series of black and brown shales, none, however, older than the Potsdam:

1st. His Swanton slates. These are the Utica and Hudson River formations which are distinctly overlapped by the Potsdam at various points along its western border, as at Sharpshins and Appletree Point, near Burlington, where the superpostion is clearly seen, and where *Triarthrus Beckii* (*Atops*), is found in the underlying Hudson River slates. An excellent section, showing the superposition of the Potsdam to the Hudson River formation at St. Albans, is given by Mr. Hitchcock in the Geology of Vermont, page 374.

2d. The Georgia slates which are but an interstratified part of the Potsdam, as appears from the section at Swanton. The

Conocephalites Teucer Billings, cited by Mr. Perry as characteristic of these supposed lower slates, is, according to Mr. B., common to the slates and to the associated Red Sandrock.

3d. The black slates to the east of the Potsdam which belong to the Levis formation, and afford its characteristic fossils at various points all along their outcrop. We have seen that the Levis formation with its graptolitic slates had been at an early date confounded with the Utica and Hudson River formations in Canada, and all of these have now been confounded with the Georgia slates to make up the Middle Taconic. Finally, it remains to be said that this view consigns to a still more remote period, the lower Taconic, the great mass of the Quebec group which lies between the lines of fault already alluded to and the Green mountains, including the white marbles or Eolian limestones with their associated fossils, the Orthoceratites, Maclureas and other fossils already named, which no paleontologist would think of assigning to a horizon below the Potsdam.

To sum up in a few words—all the evidence, paleontological and stratigraphical, as yet brought forward, affords no proof of the existence in Vermont of any strata (a small spur of Laurentian excepted) lower than the Potsdam formation, which the present advocates of the Taconic system regard as forming its summit. The supposed more ancient Middle and Lower Taconic clearly consists in part of Potsdam, in part of Utica and Hudson River, and in part of the Quebec group, which also constitutes the Lower Taconic. To the upper portion of the Quebec group, the Geological Survey of Canada have already referred the gneiss of the Green mountains, assigning to this chain a synclinal structure, nor does there yet seem to be any reason to believe otherwise.

That strata still older than the Potsdam of New York and Vermont were deposited in some portions of the oceanic area, is apparent from the existence in New Brunswick of the St. John's slates holding a primordial fauna older than the Potsdam, and it is not impossible that their equivalents may underlie the Potsdam formation of Vermont. No such rocks have, however, as yet, been detected either in Vermont or Canada, and to preserve the name of the Taconic system as the designation of a series of rocks older than the Potsdam and lying unconformably beneath it, is simply to perpetuate an unfortunate mistake which I believe Dr. Emmons, if now living, would, with the paleontological evidence at present before the world, be the first to acknowledge.

ART. XXI.—*Contributions from the Sheffield Laboratory of Yale College.* No. XVII.—*On Willemite and Tephroite;* by WILLIAM G. MIXTER, Ph.B., Assistant in the Sheffield Laboratory.

THE willemite occurring in the well known franklinite and zincite veins at Mine Hill and Stirling Hill in Sussex Co., New Jersey, has been frequently analyzed; but the results obtained by different chemists vary considerably.* This variation may be explained in part by the fact that specimens taken from near the surface are sometimes altered by the oxydation of the manganese. This is especially true of many specimens of the so-called troostite which on the exterior of the crystals are blackened and quite opaque, while the interior is often a translucent to transparent honey-yellow mineral. There is furthermore a great variation in color even of the unaltered mineral, which is found shading from almost pure white to apple-green through different tints of yellow and red.

Having recently visited the locality and obtained some remarkably fresh and unaltered specimens, I have undertaken their analyses with the following results on three different varieties.

I. *Apple-green Willemite.*—This variety is found in considerable abundance on Mine Hill, but is apparently of not so frequent occurrence at the Stirling Hill mines. It has a bright apple-green color, shading in some instances almost into white. In most specimens it is intimately mixed with franklinite, zincite and calcite, but the masses are often sufficiently large to allow of its easy selection free from these minerals. No difficulty was experienced in obtaining the green mineral perfectly pure for analysis. Specific gravity $=4{\cdot}16$ (at 17° C.) Hardness $=5{\cdot}5$. A qualitative analysis indicated besides zinc and silica, a considerable portion of manganese and traces of iron and magnesia. The mineral readily gelatinizes with acids and after the separation of the silica in the usual manner, the quantitative separation of the iron was effected by precipitating it as basic-acetate, and from the filtrate the manganese was thrown down as oxyd by oxydation with bromine, this oxyd was filtered off, washed and re-dissolved and precipitated with carbonate of soda and weighed as proto-sesquioxyd. The zinc was thrown down from the filtrate from the oxyd of manganese as sulphid, subsequently re-dissolved and precipitated by carbonate of soda. The magnesia was separated from the filtrate from the sulphid

* For analyses, see Dana's Mineralogy, 5th ed., p. 262.

of zinc as ammonio-phosphate and weighed as pyro-phosphate. Great care was taken to ensure the complete separation of the different bases and the purity of the different precipitates. Two analyses gave:—

	1.	2.	Mean.	Oxygen.	
Silica,	27·43	27·36	27·40	14·61	14·61
Zinc-oxyd,	66·83	----	66·83	13·19	14·49
Manganous oxyd,	5·69	5·78	5·73	1·29	
Ferrous oxyd,	0·06	0·06	0·06	·01	
Magnesia,	trace	trace	trace		
Ignition,	0·18	0·18	0·18		
	100·19		100·20		

II. *Honey-yellow Willemite.*—This variety occurs in hexagonal crystals with rhombohedral terminations imbedded in the highly crystalline and cleavable limestone as well as sometimes intimately associated with the zincite and franklinite. The specimen selected for analysis was translucent, almost transparent, of a honey-yellow color, and formed the interior of a crystal an inch or more in diameter, which exteriorly was of a pale yellowish to flesh-red color. The mineral was entirely free from any traces of franklinite or zincite. Specific gravity = 4·11. Hardness = 5·5. B.B. same as No. 1. Analyses conducted as before described, gave

	1.	2.	Mean.	Oxygen.	
Silica,	27·75	28·09	27·92	14·89	14·89
Zinc oxyd,	58·05	57·62	57·83	11·42	14·86
Manganous oxyd,	12·62	12·57	12·59	2·84	
Ferrous oxyd,	0·49	0·74	0·62	·14	
Magnesia,	1·15	1·12	1·14	·46	
Ignition,	0·28	0·28	0·28		
	100·34	100·42	100·38		

III. *Ash-gray mineral.*—This variety is found at the north opening on Mine Hill. Some specimens show a distinct cleavage in one direction, while on others, little or no cleavage can be observed; and although the mineral was at first suspected to be tephroite, still the decision that it was this species was not fully reached until it was analyzed. It occurs in large mases, but contains disseminated through it, minute spangles of zincite, sometimes giving it a reddish tinge and rendering it exceedingly difficult to select pure material for analysis. Specific gravity = 4.* Hardness = 5·5. B.B. the mineral is more fusible than green and honey-yellow willemite, and instead of yielding

* By a typographical error, the specific gravity of the magnesian tephroites examined by Collier and Hague is stated in my article (this Journal, II, xxvii, 66), to be 2·97 and 2·87, instead of 3·97 and 3·87.—G. J. B.

a white enamel, turns black and gives a black glass on fusion. Two analyses of the mineral selected with great care to free it from the zincite yielded:

	1.	2.	Mean.	Oxygen.	
Silica,	29·42	29·46	29·44	15·70	15·70
Zinc oxyd,	7·36	7·36	7·36	1·45	
Manganous oxyd,	57·55	57·07	57·31	12·91	
Ferrous oxyd,	0·89	0·84	0·87	·19	16·27
Lime,	2·51	2·52	2·51	0·72	
Magnesia,	2·49	2·51	2·50	1·00	
Ignition,	0·27	0·27	0·27		
	100·49	100·03	100·26		

The method employed in the analyses was essentially the same as in the other minerals, except that the lime was separated from the filtrate from the sulphid of zinc as oxalate prior to the precipitation of the magnesia as phosphate.

The results of these analyses prove the three minerals to be all true unisilicates. In I, we have the oxygen ratios of bases to silica as 14·49 : 14·61, in II, 14·86 : 14·89. The green willemite contains 5·77 per cent of manganous oxyd while the very pure honey-yellow mineral contains 12·59 per cent, thus differing in composition very materially from the willemite of European localities. This is a much higher percentage of manganous oxyd than obtained in any other analysis of the American mineral. The ash-colored mineral, as appears from the analysis, is proven to be tephroite. It bears a close resemblance to a specimen of the original tephroite received from Prof. Breithaupt by Prof. Brush, but it was first thought that it differed from this chemically in having a portion of the manganese replaced by zinc. A more careful examination of the specimen analyzed with a magnifier, shows that besides having zincite as a mechanical impurity, it also contains dissemiated through it minute specks of pale green, almost white, willemite, and it was not found practicable to select the tephroite for analysis free from this impurity. But specimens since found at the locality appear to be entirely free from this admixture. The analyses are of interest as identifying the *ash-gray* variety of tephroite originally described by Breithaupt, and which hitherto had not been re-discovered at the locality. It occurs in considerable abundance at the north end of Mine Hill, and may be readily distinguished from willemite by its ash-gray color, its pyrognostic characters, and its ordinary feldspar-like cleavage.

New Haven, June 25th, 1868.

Art. XXII.—*Notices of papers in Physiological Chemistry.* No. I.—By George F. Barker, M.D.

1. *On Hæmatoidin.*

Holm has published* the results of a research upon this substance, made in the laboratory of Professor Städeler in Zurich. The investigation was undertaken for the purpose of fixing more accurately the properties of hæmatoidin, as well as to settle the question of its identity with bilirubin. To this end, Holm first prepared this latter body, both from gall-stones and from human bile; confirming minutely all the properties previously assigned to it by Städeler.

The hæmatoidin was obtained from several sources. Apoplectic clots in the brain were first used; but the quantity thus obtained was small, permitting merely the fact of the yellow color of the chloroform solution and its change to green on exposure to light, to be noticed. The yellow masses from the ovaries of the cow were taken for a final trial. These corpora lutei contain hæmatoidin crystals, many of those figured by Funke being from this source. When an ovary is divided lengthwise, the most recent yellow bodies, which appear upon the exterior of the organ as large yellowish-red conical protuberances, are found to consist of a soft juicy mass, colored various shades of yellow, and crossed by radiating lines. The older bodies are cinnabar-red in color, dry, and granular. Well-formed crystals were rarely observed even with a magnifying power of 300 diameters; though aggregations of short needles, and irregular reddish tables were noticed, which, especially in the younger bodies, were mixed with yellow fat. Nitric acid containing nitric peroxyd tinged the mass greenish-blue, transiently.

Both chloroform and carbonic disulphid may be used for the extraction of hæmatoidin, since both yield it crystallized. Holm's process of preparation was as follows: All the red and yellow bodies were cut from the ovary with scissors, freed as completely as possible from the surrounding tissues, and rubbed to a fine pulp with pounded glass; this pulp was then placed in a flask with chloroform, and allowed to stand for several days, being frequently agitated. Upon filtration, a deep gold-yellow solution was obtained, which on spontaneous evaporation, left a semi-fluid yellowish-red fatty mass. The fat was evidently a solvent of the hæmatoidin, since it was not until after some days that crystallization began. As it proceeded, the fat gave

* J. pr. Ch, c, 142, March, 1867.

up its coloring matter to the crystal; leaving finally single and nearly colorless fat drops, each of which contained a small, but very beautifully defined hæmatoidin crystal.

If the formation of these crystals be watched under the microscope with a power of 150 diameters, they will first be seen as acute three-sided tables, one of the sides being frequently curved. Generally, two of these triangular crystals unite by their bases, thus giving the rhombic table, so characteristic of hæmatoidin. Sometimes there is an indentation at the obtuse angle of the rhomb, into which other pairs of twins may grow, producing four pointed stars. And these, by the filling in of their reëntering angles, pass into square tables, which increase in thickness and resemble cubes. The obtuse angle of the rhomb too, is often rounded.

Hæmatoidin is one of the most beautiful substances known to organic chemistry, resembling murexid very closely. By reflected light, perfect crystals show a magnificent cantharides-green color and a metallic luster; by transmitted light they appear red. If not too thick, the crystals, as viewed under the microscope, seem to be a pure magenta red; and a number of crystals irregularly aggregated together show a rich blue or violet tint.

The complete purification of these crystals from the adhering cholesterin and fat, is exceedingly difficult. After removing the greater part of the fluid fat by agitation with absolute alcohol, a tenacious fatty mass was left which dissolved only in ether. As the hæmatoidin is also soluble in this menstruum, a great loss was experienced in this process, the quantity obtained being insufficient even for an elementary analysis. A second portion not quite free from fat, was recovered from the etherial solution. This was used for the detection of iron, by incinerating it on platinum foil. A gray speck remained, which on being dissolved in chlorhydric acid, gave a slight greenish tint on the addition of potassic ferrocyanid. On account of the small quantity of substance used, and also of its not being perfectly pure, Holm is unwilling to decide upon the presence of iron in hæmatoidin.

After treatment with ether, the crystals have no longer the brilliant cantharides-green luster, their surfaces being etched by this solvent. They now resemble freshly prepared chromic acid. Moistened while in the field of the microscope, with nitric acid containing nitric peroxyd, a bright but transient blue color is developed.

Hæmatoidin is very readily soluble in chloroform, giving a gold-yellow solution; and in carbonic disulphid with a flame-red color, or orange if greatly diluted. Less soluble in absolute

ether, not at all in absolute alcohol and water, nor in ammonia, sodic hydrate, dilute mineral acids (non-oxydizing) or dilute acetic acid.

From a close comparison of hæmatoidin with bilirubin, Holm concludes :—That these bodies have not only different forms and colors, but also essentially different chemical properties. While bilirubin possesses the properties of a weak acid and combines with bases, hæmatoidin is quite an indifferent body. Bilirubin dissolves in carbonic disulphid with a gold-yellow, hæmatoidin with a flame-red color (orange, if greatly diluted). Bilirubin is insoluble in ether, hæmatoidin is easily soluble. If a bilirubin solution in chloroform be shaken with ammonia, the bilirubin is completely removed from the chloroform, which becomes colorless, while the alkaline solution becomes yellow ; hæmatoidin on the other hand, is not removed from chloroform by alkaline solutions. Indeed these bodies may thus be perfectly separated. Bilirubin shows in solutions containing alcohol, even when only the smallest traces are present, upon adding nitric acid containing nitric peroxyd, a magnificent play of colors, green, blue, violet, red and yellow, while a similar solution of hæmatoidin is simply decolorized.

2. *On the coloring matter of the yolk of eggs.*

STÄDELER* has made some experiments with the coloring matter of the yolk of the egg, in order to make a comparison between it and the bile pigment, to which it had been compared by Chevreul.

If the fresh yolk be agitated with ether, the coloring matter and the fat are dissolved, and the solution leaves upon evaporation a yellow fatty mass, which gives a dirty bluish-green color when mixed with nitric acid containing nitric peroxyd, or with a few drops of concentrated sulphuric acid. If this egg-oil be heated with a five per cent solution of sodic hydrate to saponify the fat, an unpleasant odor like that of fish livers, is developed ; and upon agitating the saponified mass with ether, it is deprived of all its coloring matter. This behavior proves the absence of bile-pigment, since the compound of bilirubin with sodium, is insoluble in ether. By evaporation, the etherial extract leaves a deep gold-yellow difficultly saponifiable fat, which by the crystallization of the cholesterin, solidifies to a butter-like mass. This fat, rich in pigment, possesses great similarity with the fat containing hæmatoidin, obtained from the ovaries. Nitric acid, when colored, develops a pure blue tint, which is transient. Ether and chloroform dissolve it to a gold-yellow solution ; this, mixed with alcohol and treated with colored nitric acid, is simply decolorized, affording no play of

* J. pr. Ch., c, 148, March, 1867.

colors. With carbonic disulphid an orange-red solution is obtained. And finally, alkalies do not remove the pigment from its solution in chloroform.

From these results, Städeler concludes that the coloring matter of the yolk of the egg is either hæmatoidin itself, or a body very similar to it in properties.

3. *On the chemical constituents of the Supra-renal capsules.*

Holm has also published* the results of a new examination of the supra-renal glands. These organs were collected fresh from the slaughter-house, and, when freed carefully from adhering fat, weighed 650 grams. They were rubbed to a fine pulp with glass-powder, mixed with twice their volume of strong alcohol, digested at a gentle heat, pressed out and filtered, the alcohol distilled off, and the remaining fluid precipitated with neutral plumbic acetate. This lead precipitate was not farther examined. The color of the filtered fluid was brownish-yellow, and the edge of the filter in contact with the air acquired a violet tint. The fluid was now precipitated with basic plumbic acetate, and allowed to stand 12 hours. The precipitate was then collected, and the filtrate treated with cupric acetate and heated to boiling, by which a second precipitate was formed, mixed with a considerable quantity of red cuprous oxyd. This copper precipitate being separated by filtration, left a purple colored fluid.

In order to extract completely the uric acid and the xanthin existing in the tissue, the pulpy mass left from the above treatment with alcohol, was digested with water at 50° C.; and the liquid thus obtained was treated successively with neutral and with basic plumbic acetate, and with cupric acetate. The basic lead precipitate and the copper precipitate were united with the similar precipitates obtained in the first process, and examined together.

(1.) *Basic lead precipitate.*— This contained no uric acid but afforded a considerable quantity of inosite, which was easily recognized by its sweet taste, easy solubility, non-precipitability by ammonic carbonate, crystalline form, and behavior with nitric acid, calcic chlorid, and ammonia.

(2.) *Copper precipitate.*—This was free from xanthin, but contained considerable hypoxanthin, easily soluble in dilute chlorhydric acid, and yielding on evaporation a salt crystallizing in needles, which, when decolorized with animal charcoal, evaporated to dryness with ammonia, and extracted with water, left pale yellow hypoxanthin. This, when dissolved in dilute nitric acid and carefully evaporated, left a scarcely yellow spot, be-

* J. pr. Ch., c, 150, March, 1867.

coming citron-yellow on stronger heating, and giving a splendid purple color on moistening with sodic hydrate.

(3.) *Filtrate.*—As already mentioned, the filtrate had a dingy purple tint. It was treated with sulphydric acid, the deposited sulphids were removed and it was then evaporated on the water-bath. During this process, the pigment separated as a violet pellicle. The fluid filtered from this, afforded by farther treatment, taurin; and beside this, a few small rounded masses were detected by microscopic examination, which may have been leucin, since Neukomm states that these glands, observed during Bright's disease afforded him this substance. On the other hand, however, Seligsohn affirms that the supra-renal capsules contain no leucin, while Virchow asserts that they contain it in large amount.

The violet pigment which separated during the evaporation of the filtrate, was found to be insoluble in alcohol, ether, chloroform, carbonic disulphid, and benzol. Alkalies took up only a trace of it, perhaps only an impurity, the filtrate being yellow, and the undissolved pigment remaining unchanged in color. Water was found to dissolve it readily when acidulated with any mineral acid; it was also dissolved by moderately dilute acetic acid, on boiling. These acid solutions were yellow; and from them the entire coloring matter separated again in violet flocks on the addition of ammonia. This pigment appears therefore, to be a weak base. Unfortunately, the quantity obtained was too small to allow of a more thorough examination; a fact which Holm regrets, inasmuch as this coloring matter, as he suggests, appears to be the pigment which produces the peculiar bronzing of the skin, which is characteristic of Addison's disease.

Arnold has stated that the coloring matter of the supra-renal capsules is precipitable by plumbic acetate. Holm maintains that there is but one chromogen in these glands, which is not precipitable by the lead salt, and which passes into the pigment by oxydation. He supports this view by the fact that the yellow alcoholic extract, which becomes red in contact with light and air, affords, after precipitation with plumbic acetate, in consequence of the farther absorption of oxygen, a violet color, even in the presence of free acids. The complete conversion, however, of the chromogen into pigment takes place only when the solution is boiled with cupric acetate; the cupric oxyd giving up a part of its oxygen and becoming cuprous oxyd.

4. *On the rational formula of Urea.*

Heintz concludes an exceedingly interesting paper upon the compounds of trioxethylenamic (triglycolamic) acid,* by com-

* Ann. Ch. Pharm., cxl, 264, Dec., 1866.

paring the constitution of its amids with that of urea. Having shown that the formula of ethylic trioxethylenamate* (triglycolamic ether) is $((\text{C}_2\text{H}_2\text{O})\text{OEt})'_3\text{N}$, it follows that trioxethylenammonamid (triglycolammonamid) prepared from it by the action of ammonia, is simply $((\text{C}_2\text{H}_2\text{O})\text{H}_2\text{N})'_3\text{N}$; being ammonia in which the three atoms of hydrogen are replaced by oxethylenammonium, $((\text{C}_2\text{H}_2\text{O})\text{H}_2\text{N})'$, a monad radical. This amid is a monacid base; though it contains four nitrogen atoms which are not directly united to oxygen, but are saturated only by bonds of hydrogen or other radicals; from which we might expect it to be tetracid.

It is a well known fact, however, that when the hydrogen in ammonia or ammonia-like bodies, is replaced by negative or oxygenated radicals, the resulting compounds are either not at all basic, or are only feebly so; indeed, this fact characterizes the class of substances termed amids. Trioxethylenamic (triglycolamic) acid for example, can unite together no more acid molecules when converted into an amid than it could before. But, while the compounds formed by the acid itself with other acids, are very feeble, none of them existing in the solid form; when the hydryl atoms of the acid are replaced by amidogen atoms, thus forming the amid, the basic power is raised in intensity and the new compound yields well crystallized substances with acids. While therefore, no more acid molecules can be held after the introduction of nitrogen atoms than before, yet the same number are now held more firmly.

There is a large class of bodies which contain more nitrogen atoms than is equal to the number of monobasic acid molecules with which the substance can unite. These bodies are called ureas; and, though they contain two nitrogen atoms in the molecule, they are all monacid. They all contain, too, an oxygenated radical. The composition of these bodies may be easily brought into comparison with that of trioxethylenammonamid (triglycolammonamid); and in the similarity there observed, Heintz finds the true explanation of the constitution of the ureas.

* (C_2H_4) being the positive bivalent radical ethylene, the negative radical corresponding to it is $(\text{C}_2\text{H}_2\text{O})''$, oxethylene (formerly glycolyl); whence oxethylenic (glycollic) acid is $(\text{C}_2\text{H}_2\text{O})\left\{\begin{matrix}\text{OH}\\ \text{OH}'\end{matrix}\right.$, oxethylenamic (glycolamic) acid is $(\text{C}_2\text{H}_2\text{O})\left\{\begin{matrix}\text{OH}\\ \text{NH}_2'\end{matrix}\right.$, and tri-oxethylenamic (tri-glycolamic) acid is

$$\text{N}\left\{\begin{matrix}((\text{C}_2\text{H}_2\text{O})\text{OH})'\\ ((\text{C}_2\text{H}_2\text{O})\text{OH})'\\ ((\text{C}_2\text{H}_2\text{O})\text{OH})'\end{matrix}\right.$$

In the first place, then, the formula should indicate the monacid character of urea; hence, it may be written $N\left\{\begin{matrix}(\text{C}\Theta)''\\(NH_4)'\end{matrix}\right.$ a formula recommended by its simplicity; or $N\left\{\begin{matrix}((\text{C}\Theta)H_2N)'\\H\\H\end{matrix}\right.$ In both cases, ammonium radicals replace the hydrogen of ammonia. Which of these formulas is the more correct?

The ureas are distinguished: 1st, by their composition; being ammonia, in which carbonyl ($\text{C}\Theta$) a bivalent negative radical, has replaced hydrogen; and 2nd, by their monacid character. Under this definition are included the compound ureas also, formed from ordinary urea precisely as ethylamine, etc., are formed from ammonia. Heintz proposes to extend this definition, and to call any ammonia-like body which contains a bivalent acid radical and which is monacid, a urea. Such a compound for example, containing not carbonyl, it is true, but the bivalent acid radical oxethylene ($\text{C}_2H_2\Theta$), would be formed by treating ethylic oxethylenamate (glycolamate), with ammonia. It would be monacid, and, if urea be written $N\left\{\begin{matrix}(\text{C}\Theta)''\\(NH_4)'\end{matrix}\right.$, would be $N\left\{\begin{matrix}(\text{C}_2H_2\Theta)''*\\(NH_4)'\end{matrix}\right.$. It stands in the same relation to trioxethylenammonamid which oxethylenamic acid (glycolamic acid or glycocoll) does to trioxethylenamic (triglycolamic) acid. But it has already been proved above, that in this tertiary amid, the negative radical replaces hydrogen which is contained within the ammonium group; and hence that its formula is $N\left\{\begin{matrix}((\text{C}_2H_2\Theta)H_2N)'\\((\text{C}_2H_2\Theta)H_2N)'\\((\text{C}_2H_2\Theta)H_2N)'\end{matrix}\right.$. Of course then, the formula of the primary amid is $N\left\{\begin{matrix}((\text{C}_2H_2\Theta)H_2N)'\\H\\H\end{matrix}\right.$, and its name is oxethylenammonamid. Since therefore, urea is analogous to this primary amid, both in composition and formation, (being produced by the action of ammonia upon urethane (ethylic carbamate)), it follows that the formula of urea is $N\left\{\begin{matrix}((\text{C}\Theta)H_2N)'\\H\\H\end{matrix}\right.$; and that this body is carbammonamid, the amid of carbamic acid, precisely as oxethylenammonamid is the amid of oxethylenamic acid (glycocoll).

* This substance has not been actually prepared; though Heintz promises experimental proof of the accuracy of the above reaction.

ART. XXIII.—*Contributions from the Sheffield Laboratory of Yale College.* No. XVIII.—*On Sussexite,* a new borate from Mine Hill, Franklin Furnace, Sussex Co., New Jersey;* by GEORGE J. BRUSH.

IN examining a specimen of a fibrous mineral, obtained at Mine Hill last year, I found that it was a fibrous silicate of zinc, and being desirous of further investigating the mineral, I requested my assistant, Mr. Wm. G. Mixter, on his recent visit to the locality, to obtain as much of the fibrous substance as possible, so that a quantitative analysis might be made of it. Mr. Mixter was fortunate in obtaining one specimen of what we at first sight took to be the fibrous silicate, but on examination of its pyrognostic characters it proved to be a new mineral, a hydrous fusible borate, reacting strongly with the fluxes for manganese. This interesting discovery led me at once to revisit the locality, and I there succeeded in obtaining enough of the new mineral to give the following characters. It is found in the franklinite vein at the opening on the north end of Mine Hill, associated with franklinite, zincite, willemite, tephroite, calcite and what appears to be a double carbonate of manganese and magnesia occurring implanted on, or imbedded in, the fibrous mineral in minute hemispherical forms; it has also associated with it a black hydrate of manganese, apparently the species manganite, and a pale pink carbonate which is probably rhodochrosite. The black manganite and the double carbonate have the appearance of being products of the alteration of the borate, since where associated with these the latter seems exceedingly friable and evidently in process of decomposition.

The pure mineral is whitish with a tinge of yellow or pink, is translucent on the edges and in thin fragments, and possesses a silky to pearly luster. The structure is fibrous, sometimes asbestiform, although in other specimens it seems to cleave much more readily in one direction than in a direction at right angles to this, yielding flat fibrous fragments. The mineral occurs in seams in calcite, sometimes with the fibers running transversely, and in other specimens quite long and parallel to the seam. The hardness is slightly above 3, scratching calcite, but not aragonite. Specific gravity=3·42.

On heating in the closed tube the mineral darkens slightly in color and yields water which reacts neutral to test-papers; but if turmeric paper is moistened with this water, then with

* The discovery of this mineral was announced in the July number of this Journal, p. 140.

a drop of dilute chlorhydric acid and afterwards dried, it assumes the red color, characteristic of boric acid, and thus shows that at least a trace of this acid is driven off with the water. In the forceps the mineral fuses in the flame of a candle (F.=2) and B.B. in O. F. yields a black crystalline mass and colors the flame intensely yellowish-green. With borax and salt of phosphorus gives a deep amethystine bead in O. F., which in R. F., becomes colorless and transparent. With soda yields a green manganate.

It is readily dissolved in chlorhydric acid, and most specimens thus treated give off a minute quantity of chlorine, showing traces of a slight alteration of the protoxyd of manganese into a higher oxyd. On evaporation to dryness and resolution in acid, minute imponderable traces of silica were found. Qualitative analysis proved the presence of boric acid, manganese, magnesia, and water, with questionable traces of zinc and soda. A fragment of the mineral moistened with sulphuric acid and held in the flame of an ordinary Bunsen burner gave, when observed through the spectroscope, the characteristic spectrum of boric acid.

The exceedingly simple composition of the mineral rendered the quantitative determination of the bases comparatively easy. The mineral being dissolved in chlorhydric acid, the excess of acid was driven off and the manganese was thrown down by bromine in the presence of an excess of acetate of soda as hydrated sesquioxyd; this was re-dissolved and precipitated as ammonio-phosphate and weighed as pyro-phosphate.* The magnesia was separated from the filtrate from the oxyd of manganese, (after it was first ascertained that this solution was entirely free from manganese) as ammonio-phosphate and estimated as pyro-phosphate. The water was determined by igniting the powdered mineral in a glass tube closed at one end, about 10 inches in length with a calibre of $\frac{1}{4}$ of an inch. The length of the tube effected a complete condensation of the water, which was deposited on the interior five or six inches from the open end, and the tube and contents on being weighed proved to have suffered a loss of less than one milligram by the ignition. The water was then dried out at the ordinary temperature in vacuo over chlorid of calcium. To make entirely sure that no boric acid went off with the water, I ignited a portion of the mineral which had previously been thoroughly mixed with about five times its weight of calcined magnesia and then covered with a layer of pure magnesia. The results of this experiment

* For details of this admirable method for the estimation of manganese see T. S. Henry, Phil. Mag., IV, xvi, 197; and W. Gibbs, this Journal, II, xliv, 216.

confirmed the water determination made by the above method. The boric acid was determined by Stromeyer's method as borofluorid of potassium. The results of the analyses are:—

	1.	2.	3.	4.	5.	6.	Mean.	Oxygen.
Boric acid,	----	----	----	---	---	31·89	31·89	22·82
Manganous oxyd,	40·08	40·20	40·01	---	---	---	40·10	9·04
Magnesia,	17·12	16·76	17·21	---	---	---	17·03	6·81
Water,	----	----	----	9·64	9·53	---	9·59	8·53
							98·61	

The analysis shows a loss of 1·39 pr. ct. doubtless due chiefly to the imperfections of the method employed for determining the boric acid. Calculating the loss as boric acid, the total amount of the acid is 33·28 pr. ct. and the oxygen ratio for $\dot{B}$, $\dot{R}$ and $\dot{H}$ is 22·82 : 15·85 : 8·53 or 3 : 2·08 : 1·12. The ratio 3 : 2 : 1, although not according precisely with the analyses, is nevertheless probably the true ratio. It requires a change of but a few tenths of a per cent of water to make this ratio. In fact, in what appeared to be a fresher and less altered specimen than that above analyzed, I obtained but 8·93 pr. ct. of water, which would change the amount of boric acid calculated as loss to 33·94 pr. ct. Correcting the oxygen to correspond to these, we have for $\dot{B}$, $\dot{R}$, $\dot{H}$, 23·27 : 15·85 : 7·94, or almost exactly 3 : 2 : 1, or considering the water basic, a ratio of 1 : 1, thus bringing out a most interesting relation between this species and native boric acid which has the formula $\dot{H}^3\dot{B}$. Sussexite may be regarded an analogous compound in which $\frac{2}{3}$ of the water is replaced by manganese and magnesia, and we may write for its formula $(\frac{2}{3}(\dot{M}n, \dot{M}g)+\frac{1}{3}\dot{H})^3\dot{B}$, or if the water be not considered basic it may be represented by $(\dot{M}n, \dot{M}g)^2\dot{B}+\dot{H}$. The former I believe to be the correct view of the composition of the mineral.

In some of its physical and chemical characters sussexite resembles the mineral Szaibelyite from southern Hungary. This mineral is found imbedded in limestone in needle-like crystals, has a hardness of over 3, a density of 3, and is a hydrous borate of magnesia. One variety analyzed by Stromeyer gave the oxygen ratio of $\dot{B}$, $\dot{M}g$, $\dot{H}$, 17 : 14·1 : 4, or of acid to bases including water of 17 : 18·1, or nearly 1 : 1, requiring but a slight change in the determination of water to make this also a mineral analogous in composition to boric acid, with which indeed it is already classified by Prof. Dana in the recent edition of his Mineralogy.* Another member of the group is Hydroboracite, a hydrous borate of magnesia and lime with the oxygen ratio of $\dot{B}$, $\dot{R}$, $\dot{H}$, 4 : 3 : 1 or $(\frac{1}{4}(\dot{C}a, \dot{M}g)+\frac{3}{4}\dot{H})^3\dot{B}$. Sussexite is at present a rare mineral, but as it occurs in a vein which is extensively

* Dana's Mineralogy, 5th ed., p. 593.

mined, there is every reason to hope that it may become more abundant. Its pyrognostic properties are so very characteristic that it may readily be distinguished from any other mineral which it resembles in physical characters. In addition to fibrous willemite, I have also found chrysotile in fine fibers imbedded in the calcite of Mine Hill; it, however, requires but little familiarity with sussexite to distinguish it at a glance from these species.

New Haven, July 18th, 1868.

ART. XXIV.—*On an easy and very effective mode of showing the vibrations in Chladni plates, &c., to a large class, by the use of a calcium or electric lantern;* by JESSE S. CHEYNEY, A. M.

THE lantern is constructed as usual, except that the condensers project some distance in front of the box, which must not be too broad; and the object-glasses are elevated upon a brass stand on the end of a sliding bar two and a half inches wide, which slides smoothly and lightly in a groove cut for it under the bottom of the lantern, and can be clamped in any position required. In my earlier experiments, I used a somewhat different arrangement of the object glasses, but the plan above, which is a modification of one devised by Prof. Albert R. Leeds, of this city, works most satisfactorily.

The lantern thus constructed, is mounted like the telescope of a theodolite, on an axis turning in a pair of vertical posts, which rise out of a circular disc of wood 22 inches in diameter. This disc is fastened loosely at its center, by a bolt with a screw, to the top of the operating table, and thus can be turned completely round in a horizontal plane, and clamped by the screw where required.

On one end of the axis of the lantern, and outside of the upright post is a circle of brass 8 inches in diameter, by which the lantern can be clamped at any angle of elevation or depression by screws set in a pair of brass clamps on the post.

A "square prism," (made either solid of glass or hollow of brass with glass sides and filled with bisulphid of carbon) is mounted so that it can be slipped into the front of the object-glass-mounting and turned upon an axis as usual.

(1.) To show the vibrations in Chladni plates, a narrow clamp, carrying suitable glass plates, is screwed to the upper part of the front of the lantern, so that the center of the plate is directly before the center of the condensers. The lantern is now turned

on its axis until the front is horizontal and the "square prism" slipped into the front of the objective, so that the light is reflected upon the screen. The plate is then focussed, sprinkled with sand, and touched as usual with the bow.

The sudden appearance of the nodal lines is very beautiful. Any figures which can be formed on glass plates can thus be shown with great ease to the largest audience.

The retarding influence of a solution of gum on the sand can be shown in action, and the curves studied in detail.

(2.) "The Magnetic Phantom" can also be most successfully shown in the same way, employing a plate coated with albumen to prevent the filings sliding too much in a body. Numerous other similar experiments, which will occur to every operator, can be readily and beautifully performed.

(3.) The waves produced by striking the surface of mercury in a suitable dish, the colors of thin plates obtained from the action of oils upon water, &c., can be readily shown by depressing the front of the lantern, without the object-glasses, and receiving the light reflected from the dish of fluid upon a proper focussing glass and thus upon the screen.

(4.) In spectrum analysis, the arrangement gives great facility, especially by mounting the prism-box described by J. P. Cooke, Jr.,* so as to slide into the front of the object tube. To keep the edge of the prisms parallel to the slit, a notch is cut in the front of the object tube and a corresponding projection screwed into the sliding end of the prism-box.

(5.) The arrangement proposed has the advantage of simplicity, and gives the greatest facility in the use of the lantern for ordinary purposes as well as in the class of experiments described.

Of course the condensers should not be subjected to the vertical current of hot air from the lime or electric light until they have been thoroughly warmed. With this precaution there is no danger.

I would remark, in conclusion, that this mode of mounting the lantern was devised in the spring, and effected, in all its essential features, in the autumn of 1867; and has been used with great satisfaction in results ever since.

Frankford, Philad., Pa., June 25, 1868.

* This Journal, II, xl, 243, Sept. 1865.

ART. XXV.—*On new specimens of Eozoon Canadense, with a reply to the objections of Professors King and Rowney;* by J. W. DAWSON, LL.D., *and notes by* W. B. CARPENTER, M.D. Illustrated by two plates.

[This paper, published in the Quarterly Journal of the Geological Society for August, 1867, is preceded by an article by Sir Wm. E. Logan, from which the following extracts are made by way of introduction.—EDS.

The most important of these specimens was met with last summer by Mr. G. H. Vennor, one of the assistants on the Canadian Geological Survey, in the township of Tudor and county of Hastings, Ontario, about forty-five miles inland from the north shore of Lake Ontario, west of Kingston. It occurred on the surface of a layer, three inches thick, of dark gray micaceous limestone or calc-schist, near the middle of a great zone of similar rock, which is interstratified with beds of yellowish-brown quartzite, gray close grained siliceous limestone, white coarsely granular limestone, and bands of dark bluish compact limestone and black pyritiferous slates, to the whole of which Mr. Vennor gives a thickness of 1,000 feet. Beneath this zone are gray and pink dolomites, bluish and grayish mica-slates, with conglomerates, diorites, and beds of magnetite, a red orthoclase gneiss lying at the base. The whole series, according to Mr. Vennor's section, which is appended, has a thickness of more than 21,000 feet; but the possible occurrence of more numerous folds than have hitherto been detected, may hereafter render necessary a considerable reduction.

These measures appear to be arranged in the form of a trough, to the eastward of which, and probably beneath them, there are rocks resembling those of Grenville, from which the former differ considerably in lithological character; it is therefore supposed that the Hastings series may be somewhat higher in horizon than that of Grenville. From the village of Madoc, the zone of gray micaceous limestone, which has been particularly alluded to, runs to the eastward on one side of the trough, in a nearly vertical position into Elzivir, and on the other side to the northward, through the township of Madoc into that of Tudor, partially and unconformably overlaid in several places by horizontal beds of Lower Silurian limestone, but gradually spreading, from a diminution of the dip, from a breadth of half a mile to one of four miles. Where it thus spreads out in Tudor it becomes suddenly interrupted for a considerable part of its breadth by an isolated mass of anorthosite rock rising about 150 feet above the general plain, and supposed to belong to the unconformable Upper Laurentian, thus showing that the specimens of *Eozoon* of this neighborhood, like those previously discovered and described, belong to the Lower Laurentian series.

The Tudor limestone is comparatively unaltered; and, in the specimen obtained from it, the general form or skeleton of the fossil (consisting of white carbonate of lime) is imbedded in the limestone, without the presence of serpentine or other silicate, the color of the skeleton contrasting strongly with that of the rock. It does not sink deep into the rock, the form having probably been loose and much abraded on what is now the under part, before being entombed. * * *

In Dr. Dawson's paper mention is made of specimens from Wentworth, and others from Long Lake. In both of these localities the rock yielding them belongs to the Grenville band, which is the uppermost of the three great bands of limestone hitherto described as interstratified in the Lower Laurentian series. That at Long Lake, situated about twenty-five miles north of Côte St. Pierre in the Petite Nation Seigniory, where the best of the previous specimens were obtained, is in the direct run of the limestone there; and like it the Long Lake rock is of a serpentinous character. The locality in Wentworth occurs on Lake Louisa, about sixteen miles north of east from that of the first Grenville specimens, from which Côte St. Pierre is about the same distance north of west, the lines measuring these distances running across several important undulations in the Grenville band in both directions. The Wentworth specimens are imbedded in a portion of the Grenville band, which appears to have escaped any great alteration, and is free from serpentine, though a mixture of serpentine with white crystalline limestone occurs in the band within a mile of the spot. From this gray limestone, which has somewhat the aspect of a conglomerate, specimens have been obtained resembling some of the figures given by Gümbel in his illustrations of the forms met with by him in the Laurentian rocks of Bavaria. * * *

It was the general form on weathered surfaces, and its strong resemblance to *Stromatopora*, which first attracted my attention to *Eozoon;* and the persistence of it in two distinct minerals, pyroxene and loganite, emboldened me, in 1857, to place before the Meeting of the American Association for the Advancement of Science specimens of it as probably a Laurentian fossil. * *

The following is from an ascending section of the Laurentian rocks in the county of Hastings, Ontario, by Mr. H. G. Vennor.

	Feet.
1. Reddish and flesh-colored granitic gneiss	2,000
2. Grayish and flesh-colored gneiss, sometimes hornblendic; mean of several pretty closely agreeing measurements	10,400
3. Crystalline limestone, sometimes magnesian, includes in Elzivir a one foot bed of graphite; sometimes very thin, but in other places attains a thickness of 750 feet; estimated as averaging	400
4. Hornblendic and dioritic rocks, massive or schistose, occasionally associated near the base with dark micaceous schists, chloritic and epidotic rocks, including beds of magnetite; average	4,200
5. Crystalline and somewhat granular magnesian limestone, occasionally interstratified with diorites, and near the base with siliceous slates and small beds of impure steatite	330

This limestone is metalliferous, holding disseminated copper pyrites, blende, mispickel, and iron pyrites, the latter also sometimes in beds of

	Feet.
two or three feet. Gold occurs in it at the village of Madoc, associated with argentiferous tetrahedrite and in irregular veins with bitter-spar, quartz, and carbonaceous matter.	
6. Gray siliceous or fine-grained mica-slates, with an interstratified mass of about sixty feet of yellowish-white dolomite	400
7. Bluish and grayish micaceous slate, interstratified with layers of gneiss, and occasionally holding crystals of magnetite	500
8. Gneissoid micaceous quartzites, banded gray and white, with a few instratified beds of siliceous limestone	1,900
9. Gray micaceous limestone, sometimes plumbaginous, interstratified below with occasional layers of diorite, and layers of a rusty-weathering gneiss	1,000
This division in Tudor is traversed by numerous N.W. and S.E. veins, holding galena in a gangue of calcite and barite. The Eozoon from Tudor, here described, was obtained from about the middle of this calcareous division, which appears to form the summit of the Hastings series.	
Total thickness	21,130]

I. SPECIMEN OF EOZOON FROM TUDOR, ONTARIO.

This very interesting specimen, submitted to me for examination by Sir W. E. Logan, is, in my opinion, of great importance, as furnishing a conclusive answer to all those objections to the organic nature of *Eozoon* which have been founded on comparisons of its structures with the forms of fibrous, dendritic, or concretionary minerals,—objections which, however plausible in the case of highly crystalline rocks, in which organic remains may be simulated by merely mineral appearances readily confounded with them, are wholly inapplicable to the present specimen.

1. *General appearance.*—The fossil is of a clavate form, six and a half inches in length, and about four inches broad. It is contained in a slab of dark-colored, coarse, laminated limestone, holding sand, scales of mica, and minute grains and fibers of carbonaceous matter. The surface of the slab shows a weathered section of the fossil (Pl. II); and the thickness remaining in the matrix is scarcely two lines, at least in the part exposed. The septa, or plates of the fossil, are in the state of white carbonate of lime, which shows their form and arrangement very distinctly, in contrast to the dark stone filling the chambers. The specimen lies flat in the plane of stratification, and has probably suffered some compression. Its septa are convex toward the broad end, and somewhat undulating. In some places they are continuous half way across the specimen; in other places they divide and re-unite at short distances. A few transverse plates, or connecting columns, are visible; and there are also a number of small veins or cracks passing nearly at right angles to the septa, and filled with carbonate of lime, similar in general appearance to the septa themselves.

On one side, the outline of the fossil is well preserved. The narrow end, which I regard as the basal portion, is rounded. The outline of the side first bends inward, and then outward, forming a graceful double curve, which extends along the greater part of the length. Above this is an abrupt projection, and then a sudden narrowing; and in the middle of the narrow portion, a part has the chambers obliterated by a white patch of carbonate of lime, below which some of the septa are bent downward in the middle. This is probably an effect of mechanical injury, or of the interference of a calc-spar vein.

With the exception of the upper part above referred to, the septa are seen to curve downward rapidly toward the margin, and to coalesce into a lateral wall, which forms the defined edge or limit of the fossil, and in which there are some indications of lateral orifices opening into the chambers. It is worthy of remark that, in this respect, the present specimen corresponds exactly with that which was originally figured by Sir W. E. Logan in the 'Geology of Canada,' p. 49, and which is the only other specimen that has exhibited the lateral limit of the form.

On the side next the matrix, the septa terminate in blunt edges, and do not coalesce; as if the organism had been attached by that surface, or had been broken before being imbedded.

2. *Microscopic characters.*—Under the microscope, with a low power, the margins of the septa appear uneven, as if eroded or tending to an acervuline mode of growth; but occasionally the septa show a distinct and regular margin. For the most part merely traces of structure are presented, consisting of small parts of canals, filled with the dark coloring-matter of the limestone. In a few places (Pl. III, fig. 1), however, these appear as distinct bundles, similar to those in the Grenville specimens, but of fine texture.

[In fig. 2 is represented a portion of the canal-system in a Grenville specimen, in which the canals, which are transparent in one side (being infiltrated with carbonate of lime only) are seen on the other to be partially filled with black matter, probably a carbonaceous residuum of the sarcode which they originally contained.—W. B. C.]

In a few rare instances only can I detect, with a higher power, in the margin of some of the septa, traces of the fine tubulation characteristic of the proper chamber-wall of *Eozoon*. For the most part this seems to have been obliterated by the infiltration of the tubuli with colorless carbonate of lime, similar to that of the skeleton.

In comparing the structure of this specimen with that of

those found elsewhere, it would appear that the chambers are more continuous, and wider in proportion to the thickness of the septa, and that the canal-system is more delicate and indistinct than usual. In the two former respects the specimens from the Calumet and from Burgess approach that now under consideration more nearly than do those from Grenville and Petite Nation; but it would be easy, even in the latter, to find occasional instances of a proportion of parts similar to that in the present example. General form is of little value as a character in such organisms; and so far as can be ascertained, this may have been the same in the present specimen and in that originally obtained from the Calumet, while in the specimens from Grenville a massive and aggregative mode of growth seems to have obliterated all distinctness of individual shape. Without additional specimens, and in the case of creatures so variable as the Foraminifera, it would be rash to decide whether the differences above noticed are of specific value, or depend on age, variability, or state of preservation. For this reason I refer the specimen for the present to *Eozoon Canadense*, merely distinguishing it as the Tudor variety.

From the state of preservation of the fossil, there are no crystalline structures present which can mislead any ordinarily skilful microscopist, except the minute veins of calcareous spar traversing the septa, and the cleavage-planes which have been developed in some portions of the latter.

I would remark that, as it seemed desirable not to injure any more than was absolutely necessary a unique and very valuable specimen, my observations of the microscopic structure have been made on a few slices of small size,—and that, as the microscopic structures are nearly the same in kind with those of specimens figured in former papers, I have not thought it necessary to prepare numerous drawings of them; while the admirable photograph executed for Sir W. E. Logan by Mr. Notman illustrates sufficiently the general form and arrangement of parts (see Pl. II).

3. *Concluding remarks.*—In a letter to Dr. Carpenter, quoted by him in the 'Quarterly Journal of the Geological Society' for August, 1866, p. 228, I referred to the occurrence of *Eozoon* preserved simply in carbonate of lime. The specimens which enable me to make that statement were obtained at Madoc, near Tudor, this region being one in which the Laurentian rocks of Canada appear to be less highly metamorphosed than is usual. The specimens from Madoc, however, were mere fragments, imbedded in the limestone, and incapa-

ble of showing the general form. I may explain, in reference to this, that long practice in the examination of these limestones has enabled me to detect the smallest fragments of *Eozoön* when present, and that in this way I had ascertained the existence of this fossil in one of the limestones of Madoc before the discovery of the fine specimen now under consideration.

I am disposed to regard the present specimen as a young individual, broken from its attachment and imbedded in a sandy calcareous mud. Its discovery affords the hope that the comparatively unaltered sediments in which it has been preserved, and which also contain the worm-burrows described by me in the 'Quarterly Journal of the Geological Society' for November,* will hereafter still more largely illustrate the Laurentian fauna.

II. SPECIMENS FROM LONG LAKE AND WENTWORTH.

Specimens from Long Lake, in the collection of the Geological Survey of Canada, exhibit white crystalline limestone with light green compact or septariiform† serpentine, and much resemble some of the serpentine-limestones of Grenville. Under the microscope the calcareous matter presents a delicate areolated appearance, without lamination; but it is not an example of acervuline *Eozoon* but rather of fragments of such a structure, confusedly aggregated together, and having the interstices and cell-cavities filled with serpentine. I have not found in any of these fragments a canal-system similar to that of *Eozoon Canadense*, though there are casts of large stolons, and, under a high power, the calcareous matter shows in many places the peculiar granular or cellular appearance which is one of the characters of the supplemental skeleton of that species. In a few places a tubulated cell-wall is preserved, with structure similar to that of *Eozoon Canadense*.

Specimens of Laurentian limestone from Wentworth, in the collection of the Geological Survey, exhibit many rounded siliceous bodies, some of which are apparently grains of sand, or small pebbles; but others, especially when freed from the calcareous matter by a dilute acid, appear as rounded bodies, with rough surfaces, either separate or aggregated in lines or groups, and having minute vermicular processes projecting from their surfaces (Pl. III, fig. 3). At first sight these suggest the idea of spicules; but I think it on the whole more

* Vol. xxii, p. 608.

† I use the term 'septariiform' to denote the *curdled* appearance so often presented by the Laurentian serpentine.

likely that they are casts of cavities and tubes belonging to some calcareous Foraminiferal organism which has disappeared.

Similar bodies, found in the limestone of Bavaria, have been described by Gümbel, who interprets them in the same way.* They may also be compared with the siliceous bodies mentioned in a former paper as occurring in the loganite filling the chambers of specimens of *Eozoon* from Burgess.

III. SPECIMENS FROM MADOC.

I have already referred to fragments of *Eozoon* occurring in the limestone at Madoc, one of which, found several years ago, I did not then venture to describe as a fossil. It projected from the surface of the limestone, being composed of a yellowish dolomite, and looking like a fragment of a thick shell. When sliced, it presents interiorly a crystalline dolomite, limited and separated from the enclosing rock by a thin wall having a granular or porous structure, and excavated into rounded recesses in the manner of *Eozoon*. It lies obliquely to the bedding, and evidently represents a hollowed flattened calcareous wall filled by infiltration. The limestone which afforded this form was near the beds holding the worm-burrows described in the Society's Journal for Nov. 1866.

[A thin section of this body, carefully examined microscopically, presents numerous and very characteristic examples of the canal-system of *Eozoon*, exhibiting both the large widely branching systems of canals and the smaller and more penicillate tufts (Pl. III, figs. 4, 5) shown in the most perfect of the serpentinous specimens—but with this difference, that the canals, being filled with a material either identical with or very similar to that of the substance in which they are excavated, are so transparent as only to be brought into view by careful management of the light.—W. B. C.]

IV. OBJECTIONS TO THE ORGANIC NATURE OF EOZOON.

The discovery of the specimen from Tudor, above described, may appear to render unnecessary any reference to the elaborate attempt made by Profs. King and Rowney to explain the structures of *Eozoon* by a comparison with the forms of fibrous and dendritic minerals,† more especially as Dr. Carpenter has already shown their inaccuracy in many important points. I think, however, that it may serve a useful purpose shortly to point out the more essential respects in which this comparison fails with regard to the Canadian specimens—with the view of relieving the discussion from matters irrelevant to it, and of

* Proceedings of Royal Academy of Munich, 1866; Q. J. G. S., vol. xxii, pt. i. p. 185 *et seq.*; also Can. Naturalist, vol. iii, p. 81.

† Quart. Journ. Geol. Soc., vol. xxii, pt. ii, p. 23.

fixing more exactly the limits of crystalline and organic forms in the serpentine-limestones and similar rocks.

The fundamental error of Messrs. King and Rowney arises from defective observation—in failing to distinguish, in the Canadian limestones themselves, between organic and crystalline forms. This is naturally followed by the identification of all these forms, whether mineral or organic, with a variety of purely crystalline arrangements occurring in other rocks, leading to their attaching the term 'Eozoonal' to any rock which shows any of the characters, whether mineral or organic, thus arbitrarily attached to the Canadian *Eozoon*. This is obviously a process by which the structure of any fossil might be proved to be a mere *lusus naturæ*.

A notable illustration of this is afforded by their regarding the veins of fibrous serpentine, or chrysotile, which occur in the Canadian specimens, as identical with the tubulated cell-wall of *Eozoon*,—although they admit that these veins traverse all the structures indifferently, and do not conform to the walls of the chambers. But any microscopist who possesses specimens of *Eozoon* containing these chrysotile veins may readily satisfy himself that, under a high power, they resolve themselves into *prismatic crystals in immediate contact with each other;* whereas, under a similar power, the true cell-wall is seen to consist of *slender, undulating, rounded threads of serpentine, penetrating a matrix of carbonate of lime.* Under polarized light, more especially, the difference is conspicuously apparent. It is true that, in many specimens and parts of specimens, the cell-wall of *Eozoon* is badly preserved and fails to show its structure; but in no instance does it present the appearance of chrysotile, or of any other fibrous mineral, when examined with care under sufficiently high powers. In my original examination of Sir William Logan's specimens from Grenville and the Calumet, I did not detect the finely tubulated cell-wall which is very imperfectly preserved in those specimens; but the veins of fibrous serpentine were well known to me; and when Dr. Carpenter discovered the tubulation of the cell-wall in the specimens from Petite Nation, I compared this structure with that of these veins, and satisfied myself of its distinctness before acceding to his conclusions on this point.

It would also appear that the radiating and sheaf-like bundles of crystals of tremolite, or similar prismatic minerals, which occur in the Canadian serpentines, and also abound in those of Connemara, have been confounded with the tubulation of *Eozoon;* but these crystals have no definite relation to the forms of that fossil, and often occur where these are en-

tirely absent; and in any case they are distinguished by their straight prismatic shape and their angular divergence from each other. Much use has also been made of the amorphous masses of opaque serpentinous matter which appear in some parts of the structure of *Eozoon*. These I regard as, in most cases, simply results of alteration or defective preservation, though they might also arise from the presence of foreign matters in the chambers, or from an incrustation of mineral matter before the final filling up of the cells. Generally their forms are purely inorganic; but in some cases they retain indications of the structures of *Eozoon*.

With reference to the canal-system of *Eozoon*, no value can be attached to loose comparisons of a structure so definite with the forms of dendritic silver and the filaments of moss-agates; still less can any resemblance be established between the canal-system and vermicular crystals of mica. These occur abundantly in some serpentines from the Calumet, and might readily be mistaken for organic forms; but their rhombic or hexagonal outline when seen in cross section, their transverse cleavage planes, and their want of any definite arrangement or relation to any general organic form, are sufficient to undeceive any practised observer. I have not seen specimens of the metaxite from Reichenstein referred to by Messrs. King and Rowney; but it is evident, from the description and figure given of it, that, whether organic or otherwise, it is not similar to the canals of *Eozoon Canadense*. But all these and similar comparisons are evidently worthless when it is considered that they have to account for definite, ramifying, cylindrical forms, penetrating a skeleton or matrix of limestone, which has itself a definite arrangement and structure, and, further, when we find that these forms are represented by substances so diverse as serpentine, pyroxene, limestone, and carbonaceous matter. This is intelligible on the supposition of tubes filled with foreign matters, but not on that of dendritic crystallization.

If all specimens of *Eozoon* were of the acervuline character, the comparisons of the chamber-casts with concretionary granules might have some plausibility. But it is to be observed that the laminated arrangement is the typical one; and the study of the larger specimens, cut under the direction of Sir W. E. Logan, shows that these laminated forms must have grown on certain strata-planes before the deposition of the overlying beds, and that the beds are, in part, composed of the broken fragments of similar laminated structures. Further, much of the apparently acervuline *Eozoon* rock is composed of such broken fragments, the interstices between which should not be confounded with the chambers; while the fact

that the serpentine fills such interstices as well as the chambers shows that its arrangement is not concretionary.* Again, these chambers are filled, in different specimens, with serpentine, pyroxene, loganite, calcareous spar, chondrodite, or even with arenaceous limestone. It is also to be observed that the examination of a number of limestones, other than Canadian, by Messrs. King and Rowney, has obliged them to admit that the laminated forms in combination with the canal-system are 'essentially Canadian,' and that the only instances of structures clearly resembling the Canadian specimens are afforded by limestones Laurentian in age, and in some of which (as, for instance, in those of Bavaria and Scandinavia) Carpenter and Gümbel have actually found the structure of *Eozoon*. The other serpentine-limestones examined (for example, that of Skye) are admitted to fail in essential points of structure; and the only serpentine believed to be of eruptive origin examined by them is confessedly destitute of all semblance of *Eozoon*. Similar results have been attained by the more careful researches of Prof. Gümbel, whose paper is well deserving of study by all who have any doubts on this subject.

In the above remarks I have not referred to the disputed case of the Connemara limestones; but I may state that I have not been able to satisfy myself of the occurrence of the structures of *Eozoon* in such specimens as I have had the opportunity to examine.† It is perhaps necessary to add that there exists in Canada abundance of Laurentian limestone which shows no indication of the structures of *Eozoon*. In some cases it is evident that such structures have not been present. In other cases they have been obliterated by processes of crystallization. As in the case of other fossils, it is only in certain beds, and in certain parts of those beds, that well-characterized specimens can be found. I may also repeat here that in the original examination of *Eozoon*, in the spring of 1864, I was furnished by Sir W. E. Logan with specimens of all these limestones, and also with serpentine-limestones of Silurian age, and that, while all possible care was taken to compare with the specimens of *Eozoon*, it was not thought necessary to publish notices of the crystalline and concretionary forms observed, many of which are very curious, and might afford materials for other papers of the nature of that criticised in the above remarks.

* I do not include here the 'septariiform' structure referred to above, which is common in the Canadian serpentine and has no connection with the forms of the chambers.

† Such Irish specimens of serpentine limestone as I have seen, appear much more highly crystalline than the beds in Canada which contain *Eozoon*.

[The examination of a large number of sections of a specimen of *Eozoon*, recently placed in my hands by Sir William Logan, in which the canal-system is extraordinarily well preserved, enables me to supply a most unexpected confirmation of Dr. Dawson's statements in regard to the occurrence of dendritic and other forms of this system, which cannot be accounted for by the intrusion of any foreign mineral; for many parts of the calcareous lamellæ in these sections, which, when viewed by ordinary transmitted light, appear quite homogeneous and structureless, are found, when the light is reduced by Collin's 'graduating diaphragm,' to exhibit a most beautiful development of various forms of canal-system (often resembling those of Dr. Dawson's Madoc specimen, represented in Pl. III, figs. 4, 5), which cross the cleavage planes of the shell-substance in every direction. Now these parts, when subjected to decalcification, show no trace of canal-system; so that it is obvious, both from their optical and from their chemical reactions, that the substance filling the canals must have been *carbonate of lime*, which has thus completely solidified the shell layer, having been deposited in the canals previously excavated in its interior, just as crystalline carbonate of lime fills up the reticular spaces of the skeleton of Echinodermata fossilized in a calcareous matrix. This fact affords conclusive evidence of *organic structure*, since no conceivable process of crystallization could give origin to dendritic extensions of carbonate of lime disposed on exactly the same crystalline system with the calcite which includes it, the two substances being mineralogically homogeneous, and only structurally distinguishable by the effect of their junction-surfaces on the course of faint rays of light transmitted through them.—W. B. C.]

EXPLANATION OF THE PLATES.

Plate II.

Specimen of *Eozoon Canadense*, imbedded in a dark-colored homogeneous limestone occurring in the Lower Laurentian series in Tudor, Ontario; two-thirds of the natural size.

Plate III.

Fig. 1. Section of one of the calcareous layers of the Tudor specimen (Plate II), showing canal-system imperfectly infiltrated with black (carbonaceous?) matter; magnified 120 diameters.

2. Section of the shelly layer of a specimen of *Eozoon* from Grenville, showing a minute form of canal-system partly injected with b
partly with serpentine; magnified 120 diameters.

3. Siliceous bodies (internal casts?) from a specimen of *Eozoon* from Wentworth; magnified 50 diameters.

4, 5. Sections of a fragment of *Eozoon* from the Madoc limestone, showing various forms of canal-system filled with carbonate of lime; magnified 120 diameters.

ART. XXVI.—*On Aquacreptite, a new mineral, and on Corundophilite of Chester;* by CHARLES UPHAM SHEPARD.

1. *Aquacreptite.*—The substance here named *aquacreptite*, on account of the crackling sound it emits when thrown into water, was sent me last autumn by Mr. W. W. Jefferis, of West Chester, Pa., with the remark that it was found at that place in a narrow seam in serpentine. The specimens are in little polyhedral fragments of the size of hazel-nuts, with flat or concave surfaces, somewhat resembling in shape the miemite from Miemo in Tuscany. Whether the mineral is an original compound, or an oxydated hydration of another substance, and if so, of what species, it is difficult to say. At present, however, it is entirely homogeneous in aspect, and possessed of much uniformity of composition. It possesses the following characters:

Massive. H.=2·5. Gr.=2·08 (Shep.), 2·05 (Eaton). Color yellowish brown. Streak orange yellow. Dull. Brittle. Fracture small conchoidal. Adheres rather feebly to the tongue. Falls asunder in water with a crackling (decrepitating) noise. The decrepitation lasts only for a moment, and is more striking if the water is warm. It does not wholly fall to powder in water; but a portion of the mass still retains a feeble degree of cohesion.

Ignited in powder, it loses 23 p. c. without change of color. Previous to ignition it is taken up by aqua regia, with exception of the silica, which separates in flocks.

Three analyses were made; 1, by Prof. James H. Eaton of Beloit College, Wisconsin; 2, by Mr. Henry C. Humphrey, a student of Amherst College, and the third by myself.

	1.	2.	3.
Silica,	43·03	41·56	41·00
Magnesia,	19·58	(not determined)	17·60
Peroxyd iron,	12·30	12·45	13·30
Alumina,	5·56	6·71	4·00
Water,	17·40	16·00	23·00
	97·87		98·90

1. *Corundophilite.*—As there had existed some perplexity in reference to this mineral as found at Chester, Mass., as the abundant associate of the emery and a constituent also of the talcose slates embracing the emery vein, I requested Prof. Eaton (then in charge of the Amherst College laboratory) to analyze the two leading varieties of the corundophilite, viz: (1) that in crystals accompanied by diaspore, rutile and sapphire

crystals, and (2) the more common variety which occurs in scales (slightly columnar), forming a narrow seam on two sides of a vein of finely granular indianite. The latter variety, as it runs for some distance in proximity to the eastern wall of the emery, has been called the "*fringe rock*" by the workmen.

Var. 1, is the same as that analyzed by Pisani, to whom also I furnished the specimens upon which he operated, and whose results I here introduce for comparison with those obtained by Eaton.

Gr. of crystallized variety as determined by Eaton =2·83.

	1.		2.
	(Eaton.)	(Pisani.)	Eaton.
Silica,	24·77	24·0	24·69
Alumina,	25·52	25·9	28·52
Magnesia,	21·88	22·7	21·86
Protoxyd iron,	15·19	14·8	16·38
Water,	11·98	11·9	(not determined.)
	99·34	99·3	

It is very noticeable that wherever at this locality the corundophilite enters into the talcose or micaceous schist it presents characters easily distinguishing the compound from all the known varieties of chlorite slate, producing in fact a new rock, whose appropriate designation will be *corundophilite slate*.

Amherst College, July 22, 1868.

Art. XXVII.—*A new locality of Meteoric Iron in Georgia;* by Charles Upham Shepard.

I am indebted to the kindness of Prof. Charles H. Hitchcock both for a knowledge of the present meteorite and for assistance in the purchase of the same from its original proprietor. It was ploughed up in April last on the farm of Mr. Michael Sullivan, two and a half miles southwest of Losttown, Cherokee county, Georgia.

It weighs six pounds and ten ounces, and has very strikingly the form of a human foot. Its color is almost perfectly black, and is wholly free from stains of iron-rust. It evinces no tendency to exfoliation; nor is it uniformly covered by a fused coating. Widmannstättian figures are visible directly in one portion of the surface. The indentations are broad and shallow, though on the whole, well pronounced.

A thin slice weighing twenty-seven grams was sawn from the heel end of the mass. The hardness proved uniform, no py-

rites having been encountered in the section. The specific gravity of the fragment is 7·52. On being etched with a dilute nitric acid, very beautiful Widmannstättian figures were presented, not quite identical with any with which I am acquainted, but most nearly resembling those of the Seneca Lake iron—the difference between the two consisting mainly in a less breadth to the bars by about one-third, in the former of these irons.

I have thus far found time only to examine the filings (or rather sawings) of this iron for sulphur and nickel. The first is wholly wanting, while the latter is abundantly present.

Amherst College, July 21, 1868.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *Hinrichs's Atom Mechanics.*—That the readers of this Journal may judge of the estimation in which Prof. Hinrichs's work on Atom-mechanics is held abroad, we translate from the German of Professor H. Fleck, of Dresden, the following notice, the original of which will be found in the *Jahrbuch für Mineralogie, Geologie*, etc., 1868, p. 333. W. G.

Under the title of "*Atom-mechanics, or Chemistry a Mechanics of the Pan-atoms*," Mr. Gustav Hinrichs, Professor of Physics, Chemistry and Mineralogy in the University of Iowa, in the United States, has published and multiplied by metallography a monograph, in which chemical processes are based upon mechanical principles, and according to which an attempt is made to refer the atomic groupings of the elements to mechanical phenomena of motion. The solution of this important problem, with which the author has been busied for twelve years, depends upon the hypothesis of an atomic system entirely new in its details, the starting point of which is *pantogen*, the fundamental principle of all chemical elements. The chemically active atoms of the elements, by the association of which chemical compounds are finally produced, are formed by the grouping of these pantogen-atoms or pan-atoms. This hypothesis it is true is not new, since in its fundamental idea it was employed by Leibnitz, Lotze, v. Wolff, Herbart and others, and has recently received from Fechner a foundation so solid and philosophically concrete, that serious doubts of the existence of primary material atoms, whether we term them monads or pan-atoms, can scarcely again be raised. But the mode of conceiving the groups of atoms as Mr. Hinrichs offers them to our consideration, with the added introduction into the calculations of the atomic weights adopted in science, is new and in every respect peculiar. Upon these last the author bases the whole essence of the phenomena of chemical change, and with their recognition Mr. Hinrichs will unquestionably obtain the reputation of a second Kepler.

The reader will have no difficulty in judging how far Mr. Hinrichs's mode of conception deserves such a recognition if we endeavor in what follows to elucidate the Atom-mechanics from the standing point of the exact sciences. After presenting the panatoms as probably existing in a free state in the extreme outer atmosphere of the sun and thus as producing light, and considering hydrogen as standing nearest to pantogen, Mr. Hinrichs expresses himself as follows: There are but two possible modes of combination for equal material points in one plane, *i. e.*, as angle-points of an equilateral triangle or of a square.

Accordingly there are but two kinds of pantogen-compounds or elements, trigonoids and tetragonoids.

This expression requires in the first place an explanation; by it Mr. Hinrichs represents the pantogen atoms as points which take their places at the angles of triangles or squares, by the association of which panatomic surfaces or atomic areas are formed, which again when superposed upon each other in number proportioned to the atomic weight of the element form the atom of an element. By this however we are involuntarily brought to the following questions, no answer to which is offered:

1. Does not Mr. Hinrichs know that the square itself may be made up of two triangles, and that consequently his tetragonoids may be referred to trigonoids?

2. If the pantogen elements form angle points in a plane, what fills up the plane and the intermediate spaces formed by the superposition of the planes? But admitting that these questions in their answers do not tell to the disadvantage of Mr. Hinrichs's ideas, the following considerations present insuperable objections: Mr. Hinrichs introduces in his calculations the atomic weights of the elements, taken double or quadruple according to the necessity of the case, under the name of Hinrichs's atomic weights, by saying that the atomic weight corresponds to the number of pantogen atoms in an elementary atom, so that if we have for instance an element with the atomic weight 100, 100 pantogen atoms are necessary for the formation of this element. These 100 atoms are distributed upon m-surfaces, in each of which there are n-pantogen points, so that $m \times n$ must be the atomic weight of the element.

Now as the author considers the trigonoids (non-metallic elements) as arising from triangles which by their juxtaposition produce an atomic surface and by their superposition a trigonoid atom, while the tetragonoid atoms (metals) are assumed to arise from the superposition of square surfaces, he fails to see the trifling weakness of making from the atomic weight of fluorine = 38, the number 35 = 5 surfaces with 7 points; from the atomic weight of bromine = 160, the number 156 = 12 surfaces each with 13 panatoms; from the atomic weight of arsenic =150 the number 152 = 8 surfaces each with 19 pan-atoms, &c., and consequently of arbitrarily altering, increasing or diminishing, as suits the figures which he has invented for the benefit of his hypothesis, the combining weights of the elements fixed by the most precise scientific

labors. If however, for the sake of a good idea we allow this arbitrary treatment of tested and recognized numerical values to pass unchallenged, we must abandon all hope and faith in it when we come to the hydrogen-atom. This element, which with its lowest atomic weight can consist at the utmost of only 4 pan-atoms, belongs to the tetragonoids and furnishes therefore only a surface but no body. Hydrogen is therefore, as the normal magnitude of Hinrichs's atomic weights most unkindly treated, and moves about in the world as a surface and without bodily attributes. This reduction of his system to a surface suggests a great superficiality in its treatment, and removes from the mind of the critic all further doubts as to the signification and value of Hinrichs's atom-mechanics. While apparently based upon mathematical principles, Mr. Hinrichs's work dispenses with a mathematical basis; though furnished with an array of chemical atomic weights, these display no true scientific treatment; though adorned with physical phrases, correct ideas of the relations of space and force are entirely wanting. Science will consequently, have nothing to expect from Prof. Hinrichs's work in its present form, and like a still-born child it will be neither mourned nor missed. Every one who has seen from the above the weak foundation upon which the fantastic air-castle of this idea of pantogen is erected, will think it most advisable to be silent as to the further development of Prof. Hinrichs's monograph in the application and utilization of his ideas.

2. *On Vortex-rings in Air;* by Professor ROBERT BALL.—The production of vortex-rings in air is experimentally shown by the interesting phenomena produced by Professor Tait. These are described by Sir William Thompson in the Philosophical Magazine for July 1867. I repeated these experiments with the kind aid of Dr. Emerson Reynolds; in doing so an interesting variation of them presented itself which renders the air-rings actually visible.

While engaged on this subject, Mr. Yeates suggested to me to try the effect of discharging an air-ring at a column of smoke; this was done, and a most curious and unexpected appearance was the result. The box employed was a 2-feet cube, and had a hole 8 inches in diameter, the side opposite having a piece of stout sacking strained over it. A blow on the sacking causes an air-ring to dart from the hole. Two large flasks containing respectively hydrochloric acid and ammonia were arranged with their mouths in close proximity and their contents vigorously boiled; from the union of their vapors a dense column of the fumes of chlorid of ammonium ascended. This apparatus was placed about eight or ten feet in front of the box. An air-ring on first leaving the box was of course invisible; when it reached the column of smoke it could be seen to force its way through it; but when it left the column and during its subsequent existence its appearance was very remarkable. At first the idea suggested itself that the single air-ring had formed two concentric smoke-rings; but closer examination and an improved column of smoke showed what had really occurred. The air-ring had penetrated the smoke quite uninjured; it had not

apparently left any of its particles behind, nor had it admitted an atom of smoke into it; but it had drawn with it sufficient of the smoke to form a complete shell, which enclosed it, and thus rendered the air visible. The phenomenon is quite in accordance that conservative tendency which theory shows must belong to a vortex-filament. The appearance is one of great beauty, and suggested the name "negative smoke-ring."

It was considered desirable to make these experiments before an evening scientific meeting of the Royal Dublin Society. Smoke-rings are seen to greatest perfection when directed along a sunbeam admitted into an otherwise darkened room; and though they can be seen beautifully in ordinary diffused sunlight, by gaslight they make a very poor show; special means of illumination are therefore necessary. The following arrangement is found to succeed admirably. By a simple optical contrivance the radiation from a brilliant lime-light is concentrated into a slightly conical beam, all other light in the room being extinguished. The box is placed in the beam, the orifice facing the lamp, and from twenty to thirty feet distant from it; at the box the diameter of the circular section of the luminous cone is from 3 to 4 feet. A smoke-ring driven from the box will, if suitably directed, traverse the beam through its entire length until it reaches the lamp; and as it is brilliantly illuminated throughout its path, the appearance presented is of great beauty. A second box can be placed at the lamp end of the beam, so as to show the effect of the collision of rings. The fumes produced by burning a small piece of phosphorus develops an ample supply of smoke in the interior of the box. This mode of producing smoke was suggested to me by Dr. William Barker; it is simpler to manage inside the box than the apparatus necessary for forming chlorid of ammonium. By the introduction of colored glasses various pleasing effects can be produced. If a sheet of tissue-paper be very lightly attached to a frame and interposed in the beam, a vigorously sent smoke-ring will sweep it away in a striking manner.

To exhibit the air-rings, the column of smoke already described is placed so as to ascend through the beam, and rather nearer the box than the lamp. An air-ring from the box is, of course, invisible till it reaches the column, while in its passage from the column to the lamp the curious phenomenon already mentioned is most beautifully shown. The negative smoke-rings are much better seen when thus illuminated than by ordinary daylight. A box of the dimensions previously given, viz: a 2-foot cube with a hole of 8 inches diameter, was found to answer better for this purpose than a somewhat smaller box with a hole of 6 inches aperture.

The column of smoke may perhaps serve as a test for the existence of vortex-rings in other cases, when, produced in air, they are invisible. This was tried in one instance. It is a common trick to blow out a candle by the puff of air from the muzzle of a gun when a percussion-cap is exploded on the nipple. This puff is probably a vortex-ring. Owing to the high velocity with which it

moves, it is difficult to trace the effect produced by the column of smoke. One person, however, firing from an elevated position and at a distance of fifteen or twenty feet from the flasks down upon the column, and another watching against a dark background, a distinct ring was occasionally seen to dart from the column after the percussion of a cap; but whether these were "negative rings," as was expected, or ordinary "smoke-rings," was not easy to determine.

Royal College of Science for Ireland, Dublin, May 28, 1868.

P. S.—Since writing the foregoing, it occurred to me to fill the box with ammoniacal gas and to discharge rings from this at a column of the vapor of hydrochloric acid. In this case the ring, before reaching the column, is perfectly invisible, and the existence of the column is only seen by slight traces of partially condensed vapor. As was expected, a beautiful ring appeared, from the combination of the two gases, when the ammonical ring reached the column.—*Phil. Mag.*, IV, xxxvi, 12, July, 1868.

June 16, 1868.

II. MINERALOGY AND GEOLOGY.

1. *Twentieth Annual Report of the Regents of the University of the State of New York on the condition of the State Cabinet of Natural History and the Historical and Antiquarian Collections annexed thereto.* Transmitted to the Legislature, April 15th, 1867. 410 pp. 8vo, with 20 plates. Albany. 1868.—This State Cabinet Report for 1867 is mainly occupied by the results of the paleontological researches of Prof. Hall. A large number of new paleozoic species are described, along with much that is of interest with regard to the structure and the relations of genera. Prof. Hall has here reproduced his very important memoir on Graptolites published by the Canadian Survey, with some additional observations.

In connection with a statement of his observations on the shell structure of some species of Spirifera, on p. 256, Prof. Hall takes occasion to complain of Mr. F. B. Meek who has published on the same subject. Mr. Meek's paper appeared in this Journal, as stated, in May, 1866. Mr. Hall, in a letter written subsequently to one of the editors of this Journal, claimed that he had had the same views for some time in print, and to show this sent on a few pages of his unpublished 4th volume. In reply, it was stated that *printing* without publishing, was no basis for a claim in science. Mr. Hall afterward had the chapter issued in the Proceedings of the American Philosophical Society, but issued without the slightest allusion to Mr. Meek's earlier publication. Mr. Meek in a later article, May, 1867, alludes in a note to this fact. Mr. Hall in reply urges that his article in the Proceedings was not published as a *paper* presented to the Philosophical Society, but as an extract from a "volume printed and not yet published," and implies that therefore he was under no obligations to allude to Mr. Meek. He

adds "that the Journal of Science professes to deal *fairly and justly* in all scientific matters, and one of its editors was in possession of all the facts and could have prevented all cause of ill feeling on the part of any one." But the editors of the Journal cannot but think that if Mr. Hall had dealt fairly in the first case, and alluded, in connection with the article in the Proceedings of the Philosophical Society, to Mr. Meek's prior publication, as he ought to have done, there would have been no occasion for any ill-feeling on the part of any one. An article published by a Society is a *paper* presented for publication, whatever may have been its previous history, and comes under the ordinary rule of courtesy among authors.

We may add that there is no man of science in the country who, in our opinion, aims to be more strictly just to others, and is more faithful in research, than Mr. Meek.

2. *On a new large Enaliosaur;* by Prof. E. D. Cope.—Prof. Cope exhibited to the Academy several fragments of a large Enaliosaur, discovered by the Academy's correspondent at Fort Wallace, Kansas, Dr. Theophilus H. Turner. Portions of two vertebræ, brought east by Dr. LeConte from his geological survey of the Pacific Railroad route, had previously indicated to the speaker the existence of an animal related to the Plesiosaurus; and the recovery of the greater part of the reptile had confirmed this affinity.

The remains consisted of over one hundred vertebræ, with numerous portions of ribs, the greater part of the pelvic and scapular arches, with two long bones somewhat like femora. Part of a muzzle, with teeth, belonged to the same animal.

The species represented a genus differing in important features from Plesiosaurus and its near allies. These were the absence of diapophyses on the caudal vertebræ, and the presence of inferiorly directed plate-like parapophyses, which took the place of the usual chevron bones in the same position; also in the presence of chevron-like bones on the inferior surfaces of the cervical vertebræ; further in some details of the scapular and pelvic arches. The diapophyses of the dorsal vertebræ originated from the centrum, and not from the neural arch.

In generic features it is related to the Cimoliosaurus and Brimosaurus of Leidy, so far as the latter are yet known. It differed from both of them in lacking diapophyses on the lumbar vertebræ.

The general form was different from Plesiosaurus in the enormous length of the tail, and the relatively shorter cervical region. The total length of the vertebral column sent was thirty-one feet ten inches, divided as follows: caudals 18 ft. 10 in., dorsals 9 ft. 8 in., cervical 3 ft. 4 in.; adding for missing cervicals and cranium at least 2 ft. 6 in., we have a total of 34½ feet. An interval of three to four feet occurred between the cervicals and dorsals as they lay in the cliff from which they were excavated, which if, as is probable, it was occupied by vertebræ in the animal, would give a length of thirty-eight feet. The caudal vertebræ had very compressed centra, and elevated neural and hæmal laminæ, and were of unusually elongate form. Neural arches everywhere on the column co-ossified.

All the vertebræ considerably more constricted medially than in Brimosaurus or Cimoliosaurus, and none except cervicals with such small antero-posterior diameter as the latter possess.

The general characters of the species are to be presented in a special essay.

He called it *Elasmosaurus platyurus* Cope, from the caudal laminæ, and the great plate bones of the sternal and pelvic regions. It was a marine saurian, whose progression was more largely accomplished by its tail than by its paddles.

The teeth and muzzle showed it to be an ally of Plesiosaurus. The former were cylindric, implanted in very deep alveolæ, and furnished with a very small pulp cavity. The exposed surface closely and sharply striate to the narrowly acuminate tip.

The beds were argillaceous, with much gypsum; the latter mineral coating the bones. The age was Cretaceous; perhaps, according to LeConte, the upper middle. The matrix beneath the dorsal vertebræ contained remains of perhaps six species of fishes, several Ctenoid, among them a known Enchodus, and a Sphyraæa, to be called *Sph. carinata* Cope.—*Proc. Acad. Sci. Philad.*, 1868, 92.

3. *Bone caves of Brazil and their animal remains;* by Prof. REINHARDT.—An important though popular paper by Prof. Reinhardt, on the bone caves of Brazil, closes with the following general conclusions:

"1. During the Post-pliocene epoch, Brazil was inhabited by a very rich Mammalian Fauna, of which the recent one might almost be said to be a mere fraction or a crippled remnant, as many of its genera, even families and sub-orders, have vanished, and very few have been added in more recent times.

2. During the whole Post-pliocene epoch the Brazilian Mammalian Fauna had the same peculiar character which now distinguishes the South American Fauna, compared with that of the old world; the extinct genera belonging to groups and families, that to this very day are peculiarly characteristic of South America. Only two of its genera, the one extinct (Mastodon), the other still living (the Horse), belong to families that in our epoch are limited to the Eastern Hemisphere.

3. All the Mammalian orders were not in the same degree richer in genera in former times than now. The Bruta, Ungulata, Proboscidea, and, lastly, the Feræ, have relatively suffered the greatest losses. Some orders, for instance the Chiroptera and Simiæ, number perhaps even more genera now than formerly.

4. The Post-pliocene Mammalian Fauna of South America differed much more from the modern one, and was especially more rich in peculiar genera, now extinct, than the corresponding fauna of the Old World.

5. The scantiness of great Mammalia—one might say the dwarf-like stamp impressed upon the South American Mammalian Fauna of our days, when compared with that of the Eastern Hemisphere, was much less observable, or rather did not exist in the prehistoric Fauna. The Post-pliocene Mastodonts, Macraucheniæ, and Toxo-

donts of Brazil, its many gigantic Armadillos and Sloths could well rival the Elephants, Rhinoceros, and Hippopotami, which during the same period roamed the soil of Europe.—*Tidschrift f. pop. Fremstillinger af Nat.*, 1867.—*The Geological Magazine, or Monthly Journ. of Geology*, p. 227.

4. *Paleozoic Entomostraca.*—Prof. T. RUPERT JONES and Dr. H. B. HOLL, publish No. VIII of notes on the Paleozoic Bivalved Entomostraca, in the Annals and Magazine of Nat. History, IV, ii, 54, (July, 1868), describing some Lower Silurian species from the chair of Kildare, Ireland.

5. *On the discovery of a new Pulmonate Mollusk* [*Zonites* (*Conulus*)* *priscus* Cpr.] *in the Coal-formation of Nova Scotia;* by J. W. DAWSON, LL.D., *with a description of the Species;* by PHILIP P. CARPENTER, M.D., (Q. J. Geol. Soc., Nov. 1867, 331.)—This memoir describes a shell found last summer in the course of excavations made under the direction of Dr. Dawson in the bed in subdivision VIII of the Joggins Section, between coals No. 37 and No. 38 of Logan's sectional list, already referred to in previous papers† as containing great numbers of shells of *Pupa vetusta*. This bed is 1217 feet below that in which *Pupa vetusta* was originally discovered in trunks of erect Sigillariæ, and about 42 feet below coal No. 37,‡ or nearly in the middle of the band of reddish and gray sandstones and shales intervening between coals No. 37 and No. 38.

In digging into the bed, it was found that the shells of *Pupa* were irregularly disposed in nests, and were in some spots very abundant, especially in the argillaceous and nodular parts, while in other places, and especially in the more carbonaceous portions, none were found. In the last-mentioned parts of the bed there are numerous obscure vegetable remains, especially leaves of *Cordaites*, leaflets of *Sphenopteris*, and *Trigonocarpa*, apparently of the same species (*T. Sigillariæ*) found with Pupæ in the original repository in the erect Sigillariæ. The appearances were such as to confirm the impression, stated in a previous paper, that the land-shells were drifted along with vegetable matter by some quiet stream, and deposited on the muddy bottom of shallow water.

One object in excavating the bed was to ascertain if any other species of land-animal than *Pupa vetusta* could be obtained from it; and in the first instance, the result appeared purely negative, except in the presence of minute fragments of bone, and of what might have been the chitinous integument of insects. On a more

* *Conulus* Fitz., 1833 (=*Trochiscus* Held, 1837, non Sly.; =*Petasia* Beck, 1837;=*Perforatella* Schlüt.), is a subgenus of *Zonites* Montf. (non Leach, Gray), according to Messrs. Adams, 'Genera of Recent Shells,' vol. ii, p. 116, and their follower Chenu, 'Manual de Conch. et de Paléont.,' vol. i, p. 422. Those who do not care to enter into the modern divisions of the land-shells, may quote the species as a Zonites—or even, speaking loosely, as a Helix.

† Quart. Journ. Geol. Soc., Feb. 1862, and May 1866, p. 121.

‡ Incorrectly stated as 12 feet in the paper last mentioned.

careful examination of the large quantity of fragments of *Pupa* obtained, I was able to select a few small specimens, all of them more or less crushed, which seemed to differ materially from the young of *Pupa vetusta* in form and surface-marking. On submitting these to Dr. Philip P. Carpenter he at once recognized their distinctness from *Pupa vetusta*. Dr. Carpenter names the species *Zonites* (*Conulus*) *priscus* Cpr.

After the description of it he adds "It is probable that there is a third species among the fragments which have been found, presenting a shape more resembling the *H. conulus*, and other trochiform snails. It would be premature however, to venture on a description until more perfect specimens have been obtained."

6. *Notes on Hetch-Hetchy Valley;* by C. F. HOFFMANN, (Proc. Ac. Sci. Cal., iii, 368.)—Tuolumne Valley, or Hetch-Hetchy, as it is called by the Indians (the meaning of this word I was unable to ascertain) is situated on Tuolumne river about fifteen miles in a straight line below Tuolumne Meadows and Soda Springs, and about twelve miles north of Yosemite Valley. Its elevation above the sea is from 3,800 to 3,900 feet, a little less than that of Yosemite. The valley is three miles long running nearly east and west, with but little fall in this distance. Near its center it is cut in two by a low spur of shelving granite coming from the south. The lower part forms a large open meadow with excellent grass, one mile in length, and gradually increasing from ten chains to a little over half a mile in width, and only timbered along the edges. The lower part of this meadow terminates in a very narrow cañon, the hills sloping down to the river at an angle of from 40° to 60°, only leaving a channel from six to ten feet wide; the river in the valley having an average width of about fifty feet. This is the principal cause of the overflow in spring time of the lower part of the valley, and probably also has given rise to the report of there being a large lake in the valley. Below this cañon is another small meadow, with a pond. The upper part of Hetch-Hetchy, east of the granite spur, forms a meadow one and three-fourths miles in length, varying from ten to thirty chains in width, well timbered and affording good grazing. The scenery resembles very much that of the Yosemite, although the bluffs are not as high, nor do they extend as far. On the north side of the valley, opposite the granite spur we first have a perpendicular bluff, the top of which is 1,800 feet above the valley; the talus at the base is about five hundred feet above the valley, leaving a precipice of about 1,300 feet. In the spring when the snows are melting a large creek precipitates itself over the western part of this bluff. I was told that this fall is one of the grandest features of the valley, sending its spray all over its lower portion. It was dry, however, at the time of my visit. The fall is 1,000 feet perpendicular, after which it strikes the debris and loses itself among the rocks. About thirty chains farther east we come to the Hetch-Hetchy fall; its height above the valley is 1,700 feet. This fall is not perpendicular, although it appears so from the front, as may be seen from the

photograph by Mr. Harris. It falls in a series of cascades at an angle of about 70°. At the time of my visit the volume of water was much greater than that of Yosemite fall, and I was told that in the spring its roarings can be heard for miles.

Still farther east we have two peaks, shaped very much like "The Three Brothers," in the Yosemite. Their base forms a large, naked and sloping granite wall on the north side of the valley, broken by two timbered shelves, which run horizontally the whole length of the wall. Up to the lower shelf or bend, about eight hundred feet high, the wall, which slopes at an angle of from 45° to 75°, is polished by glaciers, and probably these markings extend still higher up, as on entering the valley the trail followed back of and along a moraine for several miles, the height of which was about 1,200 feet above the valley. The same polish shows itself in places all along the bluffs on both sides, and particularly fine on the granite spur crossing the valley. There is no doubt that the largest branch of the great glacier which originated near Mt. Dana and Mount Lyell, made its way by Soda Springs to this valley. A singular feature of this valley is the total absence of talus or debris at the base of the bluffs, excepting at one place in front of the falls. Another remarkable rock, corresponding with Cathedral Rock in the Yosemite, stands on the south side of the valley, directly opposite Hetch-Hetchy fall; its height is 2,270 feet above the valley. The photograph by Mr. Harris will give some idea of this rock.

7. *Acadian Geology. The Geological structure, Organic remains and Mineral resources of Nova Scotia, New Brunswick, and Prince Edward Island;* by JOHN WILLIAM DAWSON, LL.D., F.R.S., etc. 2nd ed., revised and enlarged. 694 pp. 8vo, with a geological map and numerous illustrations. London, 1868, (Macmillan & Co.; Dawson Bros., Montreal.)—Dr. Dawson's Acadian Geology was originally written largely from his own researches; and the numerous additions to this second edition, are to a considerable extent from his more recent labors, although he acknowledges his indebtedness to various other sources. While he has made the rocks and coal seams of the great coal formation of Nova Scotia his special study, and devotes a large part of his volume to this subject, he has brought out much that is new with regard to all the various rock formations, from the most ancient to those of the human era. The following subjects are among those of general interest discussed in the work:

(1.) The Pre-historic Human period in Acadia, in comparison with that of Europe; (2.) The character and origin of the Boulder Clay and Surface Glaciation, in connection with prevailing theories on these subjects; (3.) The Flora of the Carboniferous period, more especially with reference to the affinities of the several genera of plants, and their relative importance in relation to the formation of Coal—a subject which will be found more fully illustrated in this work than in any previous publication; (4.) The still more curious and ancient Devonian Flora as displayed in New Bruns-

wick; (5.) The Land Animals of the Carboniferous and Devonian periods, of which Acadia has afforded so many examples; (6.) The peculiarities in the nature and age of the Auriferous Deposits of Nova Scotia; (7.) The remarkable Primordial Fauna of Southern New Brunswick, and the peculiar development of the Lower Silurian in the eastern slope of North America.

The work, while full of instruction, is therefore a treatise on many of the deeper principles of the science. The student of American Geology should study it for the new light it throws on the history of the rocks and life of the American Continent; and all interested in geological discussions will find it valuable reading. Dr. Dawson's investigations are made with thoroughness and caution, and hence his conclusions always merit consideration.

The work, besides containing a colored geological map of the region, is illustrated with numerous figures of fossils; among them several of new carboniferous reptiles (their bones and foot-prints), insects, and air breathing mollusks; a large variety of coal plants, both of the Devonian and Carboniferous, many of them novel forms, besides various species illustrating the Post-pliocene era and the life of other periods.

8. *On Subaërial Denudation, and on Cliffs and Escarpments of the Chalk and the Lower Tertiary Beds*, by Wm. Whitaker, F.G.S. 28 pp. 8vo. (From the Geological Magazine, iv, 447, 483, 1867.)—The author concludes his important paper as follows: All geologists know that rivers have made great deposits, as for instance the Wealden Beds, and therefore I do not see how they can avoid allowing that rivers, etc., have been the agents in effecting a great amount of denudation. The solid matter of the Wealden Beds must have existed somewhere before, and must have been worn away by subaërial actions and carried off by streams (the sea being quite out of the question): more too must have been worn away than was deposited afterward by the rivers, for much would be carried out to sea to form a marine deposit. Of course fresh-water beds are both less common and thinner than marine beds, but so also, as aforesaid, the comparatively trifling denudation that has formed our hills and valleys is of far less amount than that which has planed down vast tracts of country and carried off therefrom a great thickness of rock. Perhaps, indeed, the proportion that the effects of marine denudation bear to those of subaërial denudation is not far from the same as that which marine deposits bear to fresh-water deposits.

To those who say that subaërial agents are too small and too weak for the work which has been put to their credit, it may be answered that unlimited time would get over that difficulty. And it should also be borne in mind that good evidence has been brought forward that in late geological times our climate was far more severe than now, and that there may have been a far more rainy period before the present order of things was established; or in other words, that the agents in question were far more powerful than they now are in these islands. Great change indeed has

taken place in historic times; the felling of forests, the draining of land, the embanking and canalization of rivers, the reclaiming of marshes, and the like human handiworks having had their effect in lessening rainfall and floods, and therefore also the wearing action of surface water.

Lastly it seems to me that the discussion of the question of denudation has been argued on a wrong foundation. Surely if we can explain the facts and appearances we see by actions and operations that can be seen going on at the spot now, we are bound to take such explanation until it can be disproved, or until a better one can be given, and we have no right to call in the aid of other and distant operations, without there is some good sign of their having been once present (thus for instance with regard to many rock-basins now far from glaciers, there are unmistakeable signs of their having once contained ice). As a simple matter of reasoning therefore, apart from all scientific truth, we are bound to accept the theory of subaërial denudation until it can be put aside. Geologists should not call on those who hold it, and who show its agreement with things seen, to disprove other theories; but rather should expect its adversaries to disprove it, and to show first, that rain, rivers, ice, springs, damp, and frost are powerless to wear away rocks and to cut out escarpments, valleys and rock-basins, and secondly, that the sea can do and does such work. This, no light task truly, must be done, if it can be done, not by mere assertions of individual opinion, or mere statements based on hasty and prejudiced observations, but by hard work and sound reasoning. Not with us, but with our opponents, lies the *onus probandi*.

9. *Death of Fishes on the coast of the Bay of Fundy;* by Dr. A. Leith Adams, F.G.S., 22nd Regiment.—On the 24th of September, during a heavy gale from the west, impinging almost straight on to the entrance of the Lagoon known as Anderson's cove, enormous numbers of fish were observed floating dead upon the surface of the water, and thrown up in quantities by the waves. On the gale subsiding, the whole surface of the lagoon and its banks were covered with dead fish, to the depth of a foot in some places. It was evident that the shoal had been literally ground to pieces against the rocks by the force of the waves. In conclusion, the author referred to the vast quantities of fossil fish found in the Devonian and other strata, which suggested catastrophes allied to the above incident.—*Proc. Geol. Soc.*, Phil. Mag., July, 1868.

10. *Geological chart of Southern Norway, representing the dioceses of Christiania, Hamar, and Christiansand,* made during the years 1858 to 1865, by MM. Theodore Kjerulf and Telleef Dahll, under the order of the Minister of the interior of the Royal Government of Norway.—This large and beautiful chart is executed in the very best style, in all respects. It is on a scale of $1\frac{1}{2}$: 400,000. As the rocks are mainly granitic and the related metamorphic, the map has a special interest to New England and Canadian Geologists, and those of other regions where this class of rocks prevails.

11. *Revue de Géologie pour les années* 1865 *et* 1866, par M. DELESSE et M. de LAPPARENT. Vol. V. Paris, 1868. (Dunod).—Another of the valuable Annual Reports on the progress of Geology, by Delesse and Lapparent.

III. BOTANY AND ZOOLOGY.

1. *The Book of Evergreens, a practical Treatise on the Coniferæ or Cone-bearing Plants*, by JOSIAH HOOPES. Illustrated. New York. Orange Judd & Co. pp. 435, 12mo.—This book is much wanted, is creditably executed, and is likely to be widely popular. The illustrations are well chosen, numerous, and fairly good. Great pains have been taken to mention, if not to describe all the Conifers known to be anywhere in cultivation, even the tender ones which can only be kept in conservatories; and all the finer species of more favored climes which promise any chance of succeeding in this country are favorably introduced to notice, in the hope that they may somewhere in our broad land secure establishment. The trees are here treated of *con amore*, by one who not only loves them, but knows them well, in a practical and popular way.

After such general commendation, the true work of the critic might begin, and various things be pointed out, typographical, botanical, and literary, which may suggest amendment in future editions. But many of the points to be noticed have probably caught the author's eye before this. For instance, that if he must needs refer *Sequoia*, the genus of our famous Redwoods of California, to the Pine Family proper, or *Abietineæ*, he should at least modify the character of the group so as to receive it. We were at a loss to know why a genus so entirely Cupressineous as *Sequoia* should ever have been referred to *Abietineæ;* but we find that Endlicher was misled by Sir Wm. Hooker's figure of some foliage which he guessed might belong to the great tree of which Douglas and Coulter had spoken, though it is now evident that the trees they saw were of the common Redwood (*S. sempervirens*), and the branches Hooker figured, belonged to a Fir. So Endlicher, who in his latter days only compiled, mixing up in his new genus a Redwood with a Fir, attributed scaly buds to it, and referred the mixture to the *Abietineæ*; and so in his turn misled our present author, who, although aware that Endlicher's *Sequoia gigantea* is a nonentity and a blunder, did not work his way clear of the consequences. At length, when Endlicher's genus really got its second species, and Torrey and Decaine, independently at about the same time, identified it, they only followed the rule of nomenclature in transferring Lindley's specific name of *gigantea* from *Wellingtonia* to *Sequoia*, the homonym of Endlicher, taken from Hooker, having been obliterated in the correction of the error.

We must complain of Mr. Hoopes, that in his compilation of the popular account of this famous tree, he cites too much from the fanciful narrative of Bayard Taylor, and too little from the faithful account of Dr. J. M. Bigelow, and still more for his iteration of

an unwarranted statement that, "according to the annual rings, the age of this tree was 3,100 years"; and this in the very face of Dr. Bigelow's statement before him, of two actual countings of the layers, reducing this age more than half. If further evidence be required, we would refer to the account of a third and very precise measurement and counting, by Mr. De la Rue, which is referred to in this Journal for July, 1866. Notorious as is the incorrectness of the long-current popular statement, it seems impossible to arrest it. In Prof. Brewer's account of the Mariposa Grove, printed by Sir William Hooker, the name was accidentally given as "Maipula"; and so it reads in Mr. Hoopes' extract. Since Mr. Meehan is thanked for constant aid rendered, we could wish that he had informed our author, that *Pinus pungens* inhabits his own state of Pennsylvania as far north as Reading. "Nubiginean Podocarpus," as the English of *P. nubigena*, may set some readers in search of a country whence the species comes, and which they may rightly conclude is *in nubibus*. Imperfections and little mistakes are unavoidable in a first essay even of an experienced writer; but good success at length rewards faithful endeavors. A. G.

2. *The Miscellaneous Botanical Works of* Robert Brown, [edited by J. J. Bennett, for the Ray Society], vol. ii, containing 3, Systematic Memoirs, and 4, Contributions to Systematic Works. 1847. 8vo, pp. 786.—Referring to our former notice of the first volume for some account of this excellent undertaking, we may here barely state that the systematic memoirs, here given, begin with that on *Proteaceæ*, followed by the memoir on *Asclepiadeæ* and the *Apocineæ*, published in 1811 by the Wernerian Society of Edinburgh, copies of which are very rare, and end with the paper on *Limnantheæ*, 1833.

In the part entitled contributions to scientific works, we have all that Mr. Brown wrote for the later volumes of the *Hortus Kewensis*, for Richardson's Botanical Appendix, Wallich's *Plantæ Asiaticæ Rariores*, Bennett's *Pl. Javanicæ Rariores*, with extracts from *Bot. Mag.*, *Bot. Reg.*, etc., etc. The whole concludes with a capital Index of Subjects to vol. 2, by the Editor, and an Index of Names of Plants to both volumes, by Mr. Carruthers. A. G.

3. *The Journal of the Linnean Society, Botany*, numbers 42 and 43 just received, interest us chiefly in the paper entitled *Notes on Myrtaceæ*, by George Bentham, the President of the Society.—We have here an elucidation of the principles according to which this family was revised in the new *Genera Plantarum*, and the Australian species worked up in the *Flora Australica*, along with some recent corrections. Among the latter is the announcement that *Nelitris* of Gættner is not the Myrtaceous genus it was supposed to be, (viz.=*Decaspermum* Forster), but the Rubiaceous *Timonius*, as ascertained by Thwaites several years ago. In view of the unwillingness of the authors of the new Genera Plantarum to recognize genera proposed by the predecessors and contemporaries of Linnæus, it may not be superfluous to claim that the now established name of *Timonius* ought not to be superseded simply be-

cause Linnæus happened to overlook the genus altogether. That was the fault of Linnæus, not of Rumphius.

We read with much satisfaction the convincing arguments by which Mr. Bentham vindicates the orthodox view of the calyx-tube of the Rose, of various *Myrtaceæ* and the like, as against the notion lately threatening to prevail, that the consolidated lower part is a hollow receptacle or axis. Finally, as a general term for leaves and their homologues is wanted, one which may play its part in botanical description equally in Latin or any modern vernacular (which "leaf organs," suggested by Mr. Bentham, will hardly do), we propose the word *phyllon*, plural *phylla*, as every way convenient and serviceable,—in this following the analogy of *phyton* already appropriated for the integer of the axis *plus* the *phyllon* or *phylla* it bears.

At this moment we receive Nos. 44 to 47 inclusive. The most interesting and largest papers are those of Mr. Berkeley and Dr. Curtis, which, under the title of *Fungi Cubenses*, describe Mr. Charles Wright's matchless collection of *Fungi* of that island, consisting of 888 species, of which about 66 per cent are peculiar to Cuba, and the greater part new. Also Mr. Darwin's paper on the Hybrid-like nature of the offspring from the illegitimate unions of dimorphic and trimorphic plants; and on the specific differences between the primrose and the cowslip, with proof of the hybrid nature of the common oxlip. A. G.

4. *Théorie de la Feuille*, par M. Casimir De Candolle.—A short, very interesting article in the *Archives des Sciences* of the *Bibliothèque Universelle* of Geneva for May, 1868, and separately issued. A new study of the development and arrangement of the vascular bundles or woody system of the leaf, with a view to a better comprehension of the morphology of the organ. The author concludes that, as a matter of fact, the vascular bundles are arranged on the same general plan in leaf and axis, and theoretically that the leaf is a branch with apex atrophied, thus stopped from continued extension, and with posterior (upper) side atrophied, as shown in the ordinary deficiency of vascular bundles in that region, but that many leaves exhibit a strong tendency to complete the development of that side. All this will not be generally comprehended without some details, which we cannot now enter into. We should prefer to formulate these ideas somewhat differently, and identify the leaf, not with a branch or secondary axis, nor properly with axis at all, but to regard it as the expanded termination, or free portion, of each integer of axis (internode), *i. e.*, the free part of a *phyton*.

The provision in the vascular systems for very various secondary developments, posterior or facial as well as lateral, is interestingly worked out and applied to deduplication or chorisis, which thus gets new support from organogeny; and the application of investigation to the cup of the Rose and the stamen and carpel of Magnolia is not less interesting. A. G.

5. *Obituary announcement.*—It should here be recorded that two veteran botanists have recently deceased in Great Britain, both of whom had many friends in this country to deplore the loss; viz.: Nathaniel B. Ward, of Clapham Rise, near London, the inventor of the *Ward Case*, and Prof. George A. Walker Arnott of Glasgow, formerly much associated in botanical work with the late Sir Wm. Hooker, and of late very high authority upon *Diatomaceæ*. Being unable at present to prepare fitting biographical notices, we hope to do so at the close of the current year. A. G.

6. *Note on the Polymorphism of the Anthozoa, and the Structure of the Tubiporæ;* by A. Kölliker.—The polymorphism of individuals, so remarkable among the Acalephæ, has had nothing corresponding to it among the other Cœlenterata; it is therefore a very unexpected discovery that M. Kölliker has lately made, of a true polymorphism in various genera of Anthozoa Alcyonaria. This polymorphism consists in the existence, besides the large individuals capable of taking nourishment and furnished with generative organs, of other, smaller, asexual individuals, which appear essentially to preside over the introduction of sea-water into the organism, and then over its expulsion, and which are perhaps at the same time the seat of an excrementitial secretion. Like the others, these asexual individuals possess a body-cavity divided into chambers by eight septa, and a pyriform stomach with two orifices. On the other hand they are entirely destitute of tentacles; and instead of the eight ordinary mesenteric filaments there are only two, supported upon two consecutive septa. The cavity of the body of these individuals is always in communication with that of the sexual individuals; but the mode in which this communication is effected is liable to vary with the genera.

We may distinguish two types in the mode of distribution of the asexual individuals upon the polyparies. In the first they are distributed in great abundance over the whole polypigerous region of the polypary, among the sexual individuals. This is the case in certain Alcyonids which M. Kölliker refers to the genus *Sarcophyton*, and also in *Veretillum*, *Lituaria*, *Cavernularia*, and *Sarcobelemnon*. In the second case the asexual individuals are restricted to certain perfectly definite places, which, however, are variable according to the genera. Thus in certain species of *Pteroeides* they occur on the lower surface of the pennate leaves of the region serving for attachment, in the form of a larger or smaller plate; in other species of the same genus they are also found at the apex of the polypary; in the *Pennatulæ* the varicosities of the trunk correspond to the places where the asexual individuals are seated; *Funiculina quadrangularis* shows them arranged in longitudinal rows between the sexual individuals; lastly, the *Virgulariæ* always present behind each lamella, upon their trunk, a simple transverse row of asexual individuals.

It is probable that all the Pennatulidæ present a similar dimorphism; at least, in *Renilla* we see, between the fully developed polyps, rudimentary bodies which seem to be individuals of a dif-

ferent form. On the other hand, with the sole exception of the genus *Sarcophyton*, M. Kölliker has sought in vain for dimorphism in the Alcyonidæ and Gorgonidæ. It must not be forgotten, however, that there seem to exist some relations between the buds of the sexual and asexual individuals in the polymorphic polyparies; for, in *Veretillum* at least, the asexual individuals seem, under certain conditions, capable of being transformed into sexual individuals.

M. Kölliker has also been able to investigate a polypary of *Tubipora*, still enveloped by the soft parts, obtained from the Fiji archipelago. Notwithstanding the great resemblance of the polyparies of the *Tubiporæ* to those of the Madripores, the author considers that in their whole structure and development these polyps are Alcyonaria which must occupy a place by the side of the genus *Clavularia*. Both the tentacles and the body of the polyps of *Tubipora* contain spicules.— *Würzburger Zeitung*, January 4, 1868; abstract by E. Claparède in *Bibl. Univ.* February 15, 1868, *Bull. Sci.* p. 171.—*From the Ann. Mag. Nat. Hist.*, IV, i, 227.

7. *A Guide to the study of Insects, and a Treatise on those injurious and beneficial to Crops*, for the use of Colleges, Farm-schools, and Agriculturists; by A. S. Packard, Jr., M.D. With upwards of 500 engravings. Salem, 1868. Part I, June; II, July, and other parts to appear for the present, monthly.—A work of real scientific merit, combining an account of the structure of insects, with facts of practical and popular interest, and abundantly illusted, by one of the best entomologists of the country.

IV. ASTRONOMY.

1. *Notice of new Planets* (98), (99) *and* (101); by James C. Watson, of the Observatory at Ann Arbor, Michigan. (Communicated for this Journal.)—Planet (98) was discovered by Prof. C. H. F. Peters at Clinton, N. Y., on the 18th of April, in the following place:

Ham. Coll. M. T.	α	δ
April 18, $11^h\ 45^m\ 13^s$	$11^h\ 17^m\ 30^s{\cdot}89$	$-1^\circ\ 38'\ 12''{\cdot}7$

Planet (99) was discovered by M. Borelli at Marseilles on the 28th of May. His observation was:

Marseilles M. T.	α	δ
May 28, $10^h\ 26^m\ 51^s$	$13^h\ 24^m\ 7^s{\cdot}92$	$-9^\circ\ 5'\ 49''{\cdot}1$

The planet was then of the 13th magnitude. The notice of the discovery of (100) by me on July 11th has already been communicated to your Journal. I have yet to add that I discovered Planet (101) on the 15th inst. The following places were obtained with difficulty through passing breaks in the clouds:

Ann Arbor M. T.	(101) α	(101) δ
1868. Aug. 15, $12^h\ 7^m\ 38^s$	$23^h\ 53^m\ 39^s{\cdot}61$	$-0^\circ\ 48'\ 39''{\cdot}2$
15, 14 13 27	23 53 36 ·43	0 48 24 ·7
15, 14 46 54	23 53 35 ·68	0 48 20 ·0
20, 13 13 55	23 50 30 ·99	−0 37 38 ·9

The planet shines like a star of the 10th magnitude.

2. *Discovery of another new Planet by Dr. Peters.*—The Utica Morning Herald contains the announcement, dated Aug. 24th, that Dr. C. H. F. Peters discovered at the Observatory of Hamilton College, Clinton, N. Y., another new planet on Saturday night, the position of which he verified on Monday morning (the 24th). It is in the constellation of Pisces, and had at 3 o'clock of that morning 18° 38′ of right ascension, and 12° 54′ declination, moving slowly to the east. It is now equal to a star of about the eleventh magnitude.

3. *Elementary Lessons in Astronomy;* by J. Norman Lockyer. 348 pp. 16mo, with numerous illustrations. London. 1868. (Macmillan & Co).—Mr. Lockyer has prepared a very convenient and beautifully illustrated work for instruction in the elements of astronomy. The arrangement is different from usual, as the volume opens with the fixed stars and the general system of the universe, and afterwards takes up the special case of the solar system; and in this it has some marked advantages. After chapters on the stars, sun, solar system, it takes up the apparent movements of the heavenly bodies; measurements of time, etc.; telescopic and spectral observations; determination of the apparent places of the heavenly bodies, and their real distances and dimensions; universal gravitation. The illustrations are in superior style. The frontispiece contains the colored spectra of the sun, stars and nebulæ. The beautiful steel plates, one of them a view of a lunar volcano, were furnished the author by W. de la Rue.

4. *Tables for the mutual conversion of Solar and Sidereal time*; by Edward Lang, F.R.S.E. 326 pp. 12mo. Edinburgh, 1868. (Wm. Blackwood & Sons).—These tables will prove a great convenience in astronomical computations. The values are given for each tenth second through the day; and precision may thence be readily obtained to the hundredth part of a second. Moreover, tables for the values of each hundredth part up to ten seconds are prefixed, and still further to accommodate the astronomer, the values are given for each solar and sidereal day during the year. The type is large and clear, and the volume is handsomely printed.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Seventeenth Meeting of the American Association for the Advancement of Science, held in Chicago, Illinois, August* 5–12, 1868.—The Chicago meeting of the American Association, under the presidency of B. A. Gould, was in many respects one of the most satisfactory which has been held. The attendance was large, over two hundred members coming from a distance,while about as many more were added from Chicago and its vicinity. The list of papers entered exceeded one hundred and fifty titles, covering a great variety of subjects in all departments of physical science and Natural History. If few new or startling discoveries were announced, the evidence offered of zealous and successful labor in various fields of research was most satisfactory. The spirit of the

occasion was that of work and an earnest purpose to advance science. But little vague or unmeaning discussion was tolerated or offered.

A number of papers were entered touching more or less directly the question of the antiquity of the human race. Of these by far the most important was that of Prof. J. D. Whitney on the fossil human skull of Calaveras county, California. The evidence as to this alleged discovery has already been given substantially in this Journal, II, vol. xliii, p. 265, and on this point nothing of importance was added. The skull was found it is alleged, (on the evidence of the miner who owns the 'claim' and who says he himself threw it out,) in a shaft sunk in the auriferous gravel of Bald Mountain, not far from Angels Camp in Calaveras Co., Cal. in Sept. 1866, at a depth of about 130 feet and beneath several beds of volcanic matter interstratified with auriferous gold. Its true character was not recognized at the time it was thrown out, a nearly shapeless mass from the agglutination of calcareous matter and gravel impacted especially at the base of the skull. As soon as its true nature was seen it became the object of attention, and was, by Mr. Matteson its discoverer, placed with Dr. Jones of Murphy's, who is well known in that district as a zealous collector of all things rare and curious. This collector soon communicated it to the office of the Geological Survey. This story has been reported to several persons by the original discoverer with essentially the same details. The shaft meantime was filled with water almost immediately after the skull was thrown out, and has never since been pumped out or exhumed, and stands to-day as it was at the date of the alleged discovery.

The important point in Prof. Whitney's communication of these facts at Chicago was the statement of the results obtained by Dr. Jeffries Wyman, of Cambridge in the critical examination of this skull. On clearing away the mass of calcareous tufa which filled the cavity of the zygomatic arch, there were taken out two metatarsal bones, the lower end of a left fibula, part of the ulna end of a sternum bone, all of which may have belonged to the same skeleton with the skull; also a fragment of a human tibia too small for this skeleton, and a shell of *Helix mormonensis.** There was also associated with them a piece of shell fashioned to a flat disc and bored at the center, and a fragment of charcoal.

The gravel was also cleared away from the base of the skull, and the lower jaw isolated and cleaned. The skull had been fractured by violence, with the loss of the left frontal and posterior portion. The teeth and alveolar processes indicated it to belong to an old person. The frontal region was large; but whether the skull was long or broad is not certain. The orbits of the eyes were unsymmetrical, due, as was believed, to the absorption of the alveolar processes of the left jaw producing a depression on that side. These are, substantially, the main points of the case as it now stands. The geological horizon to which it is referable, should it

* *H. Mormonensis* is a species of Helix now living in Calaveras county.

turn out, as the authors believe it will, that the skull was a genuine "find" in the place alleged, is stated by Whitney to be Pliocene or Post-pliocene.

In this connection Prof. Silliman exhibited to the Association one of four molar teeth of Mastodon found in the same, or nearly the same, geological horizon as the alleged Calaveras skull. These teeth, of the origin and position of which there is no doubt, are the same mentioned in an article in this Journal by Prof. S. for May last (vol. xlv, p. 378.) They carry the Mastodon down to a lower horizon than has before been assigned to this mammal in California. It has been the belief hitherto that the great catastrophe of the volcanic outpourings which buried the Table mountains of Calaveras and Tuolumne counties in California had extinguished all the preëxisting races, and that the Mastodon had never been certainly discovered below that horizon. This view is no longer tenable, and the Mastodon is here conclusively shown to reach quite to the base of the deep-lying gold placers; and if the Calaveras skull stands the test of subsequent investigation man was his companion in these early days.

Prof. W. P. Blake, in the same connection, exhibited drawings of stone implements, and stated facts of their alleged coëxistence with fragments of human crania in the deep-lying gold placers. All Prof. Blake's statements were based on specimens and information obtained from Dr. Snell, of Sonora in California, who imparted them to Prof. B., who had not in person obtained them. Here again we lack the evidence so essential to establish unquestionable authenticity.

Among the evening proceedings should be specially noticed the Address of Pres. F. A. P. Barnard, the presiding officer at the Buffalo meeting, which, owing to the absence of the author in Paris last year, was not delivered at Burlington in due course, and was consequently given on the evening of the opening at Chicago. It was characterized by that encyclopedic comprehensiveness and interest which have rendered Dr. Barnard's addresses so favorably known.

The eulogy on Alexander Dallas Bache, by the President, Dr. B. A. Gould, was delivered on the evening of Aug. 6th, at Library Hall, and was worthy of the reputation of its author. It was a noble tribute to the life and services of the lamented scientist, who, more than any man of his time in the United States, combined with wonderful administrative ability the highest scientific attainments, and whose name and memory are cherished by all who cultivate science, whether in America or abroad.

To the labor of the Local Committee at Chicago, the Association is indebted for the enjoyment of an occasion which will be long remembered with delight by all those had the privilege of partaking of the all but universal hospitalities, which left no moment of the day or evening unemployed by the most agreeable social and intellectual pleasures. Not content with providing for the personal comfort and entertainment of their numerous guests while within

the walls of this wonderful city of the northwest, the Committee arranged for a number of excursions, after the adjournment, to various, and even very distant points of scientific interest,—to the coal-fields of Illinois—the lead mines of Galena and Dubuque—the copper regions of Lake Superior, and, more than all, to the Rocky mountains and the present terminus of the great Pacific Railroad, not less than 1,250 miles from Chicago, on the path to San Francisco. Return tickets were also provided for all who had to pass over most of the long routes of 1,000 miles to the Atlantic. Surely the members of the American Association for the Advancement of Science, and their ladies who were so fortunate as to accompany them, have good occasion to remember with pleasure the more than princely entertainment and magnificent hospitalities of Chicago.

The next meeting of the Association will be held in the city of Salem, Massachusetts, commencing August 18, 1869, under the Presidency of Col. J. W. Foster, of Chicago. B. S.

The following is a list of the papers read at the meeting:

1. In General Session.

1. On the Application of Electricity to the Maintenance of the Vibrations of the Tuning-fork; and of the Tuning-fork to the Excitement of Vibrations in Cords and Threads; Joseph Lovering.
2. Steam Boilers and the various Causes assigned for their Explosions, illustrated by Facts, Drawings and Experiments; Joseph A. Miller.
3. On the Artistic Evidence of the Remote Colonization of the North-Western or American Continent by Maritime People of Distinct Nationalities before the Modern Era; J. H. Gibbon.
4. Vestiges of Pre-historic Races in California; W. P. Blake.
5. The Antiquity of Man in North America; J. W. Foster.
6. The Fossil Human Skull of Calaveras County, California; J. D. Whitney.

2. In Section A.

Mathematics, Physics, and Chemistry.

1. On the Theory of Luminous Hydrocarbon Flames; Eugene W. Hilgard.
2. The Statics of the Four Types of Modern Chemistry, with Special Regard to the Water Type $\left.\begin{matrix}H\\H\end{matrix}\right\}O$; Gustavus Hinrichs.
3. A New and General Law, determining the Atomic Volume and Boiling Point of a great number of Carbon Compounds; Gustavus Hinrichs.
4. The Calculation of the Crystalline Form of the Anhydrous Carbonates, Nitrates, Sulphates, Perchlorates, Permanganates, and other Salts of the Composition AB_3C, or AB_4C; Gustavus Hinrichs.
5. A New Physio-Geographical Explanation of the Tides, etc.; Theodore C. Hilgard.
6. Some Experiments on the Influence of the Physical Condition upon the Personal Equation, in Transit Observations; W. A. Rogers.
7. A New Formula for the Reduction of Observations in the Prime Vertical, analogous to the Formula for the Reduction of Meridian Observations; W. A. Rogers.
8. On the Chemico-Geological Relations of the Metals; T. S. Hunt.
9. Influence of the Moon upon the Weather; Elias Loomis.
10. The Recent Contributions of Science to the Arts of Dyeing and Printing Woollen Tissues; John L. Hayes.
11. Meteorolites from Mexico and Poland; Louis Feuchtwanger.

12. On the Combining Power of the Chemical Elements; S. D. TILLMAN.
13. Further Notice of Experiments on Snow and Ice at a Temperature below 32° F.; EDWARD HUNGERFORD.
14. On Hansen's Theory of the Physical Constitution of the Moon; SIMON NEWCOMB.
15. Resuscitation of the Cincinnati Observatory; CLEVELAND ABBE.
16. The Source of Free Hydrochloric Acid in the Gastric Juice; E. N. HORSFORD.
17. Relations of the Metamorphoses of the Phosphates to Waste and Repair; E. N. HORSFORD.
18. Economy in the Conversion of Beef into Food; E. N. HORSFORD.
19. Phosphoric Acid a Constituent of Butter; E. N. HORSFORD.
20. Source of Muscular Power; E. N. HORSFORD.
21. Fluorine a Constituent of the Brain; E. N. HORSFORD.
22. The Hot Term of July, 1868; O. N. STODDARD.
23. The Principles of Statistics as applied to the Census; F. B. HOUGH.
24. Remarks on the Galvanic Battery; G. W. HOUGH.
25. On a proposed new Mechanism for the study of Galvanic Batteries; G. W. HOUGH.
26. Remarks on the total Disturbance of the Barometrical Column; G. W. HOUGH.
27. On the Mathematical Investigations made for the Construction of the Illinois and St. Louis Bridge; WILLIAM CHAUVENET.
28. On a Method of Measuring very Small Rectilinear Motions; WILLIAM CHAUVENET.
29. On the Laws of Ocean Currents; J. S. GRIMES.
30. Hough's Barometrograph as applied to the Investigation of the Storm Curve; J. H. COFFIN.
31. The Profiles of Blast Furnaces; THOMAS EGLESTON.
32. The Nature of Electric Discharge; O. N. STODDARD.
33. On the Evaporative Power of the Sun near the Base of the Sierra Nevada, in Calaveras County, California; BENJAMIN SILLIMAN.
34. On the Relation Between the Atomic Value of Different Metals, and their Paramagnetic and Diamagnetic Properties; P. H. VANDER WEYDE.
35. On the Molecular Arrangement of the Inorganic Acids; GEORGE F. BARKER.
36. Theory of the Prediction of Star Places; T. H. SAFFORD.
37. Remarks on the Secular Variations of the Eccentricities and Perihelia of the Eight Principal Planets; J. N. STOCKWELL.
38. Description and Application of the Heliostat, and Method of Running True Meridian Lines in Surveying; MICHAEL W. MCDERMOTT.
39. Tides in Lakes; H. A. NEWTON.
40. On the Tides of Lake Michigan; W. FERREL.
41. Notes on the Defects of Lightning Rods; JAMES BUSHEE.
42. Some New Facts and Views concerning Aluminum; HENRY WURTZ.
43. The Application of Carbonic Acid Gas, in the Extinguishment of Fire; E. L. BUTTRICK.
44. Atomic Motion; H. F. WALLING.
45. The Higher Law of Correlation; O. N. STODDARD.
46. The Periodic Law in the Failure of Harvests and Inundations, with suggestions as to their Insurance; GEORGE A. LEAKIN.
47. On Methods of Amalgamation in the Treatment of Gold Ores; BENJAMIN SILLIMAN.
48. On Redemption Periods of Monetary Values which involve Life Contingencies; E. B. ELLIOTT.
49. On the Metrical Unification of International Coinage; E. B. ELLIOTT.
50. Exhibition of a New Selenograph; JEROME ALLEN.
51. On a New Method of Measurement Map Drawing for Schools; JEROME ALLEN.
52. Note on Epitrochoidal Teeth; WILLIAM WATSON.

3. In Section B.

Geology and Natural History.

1. On the Genus of extinct Sea-Saurians, *Elasmosaurus;* Edward D. Cope.
2. Phases of Glacial Action in Maine at the Close of the Dritt Period; N. T. True.
3. Bibliography of Entomology in the United States and Canada, since 1862; John G. Morris.
4. On the Leaves of Coniferous Plants; Thomas Meehan.
5. On the Geology of the Mississippi Delta, and the Salt Deposit of Petite Anse; Eugene W. Hilgard.
6. Effects of Atmospheric Changes on the Eruptions of the great Geyser of Iceland; P. A. Chadbourne.
7. On the Boulder Field in Cedar County, Iowa; Rush Emery.
8. On the Upper Silurian and Devonian Rocks of Ontario; T. S. Hunt.
9. On Gold in the Laurentian Rocks of Canada; T. S. Hunt.
10. On the Gold Region of Nova Scotia; T. S. Hunt.
11. Origin of Prairies; J. W. Foster.
12. Exhibition of the Crania of Bootherium and Castoroides, with Remarks on their Geological Position and their Living Analogues; J. W. Foster.
13. On the Stratigraphical Relations of the Fossil Horse in the United States; Charles Whittlesey.
14. Brief Remarks on the Botany, Meteorology, and Geology of Mount Mansfield, Vermont; James Hyatt.
15. The Habitable Features of the North American Continental Plateau near the Line of 35° Parallel North Latitude; containing a General Summary of Conclusions derived from a Review of its Aboriginal Population and Natural Features; C. C. Parry.
16. Description of a New Species of *Protichnites* from the Potsdam Sandstone of New York; O. C. Marsh.
17. Notice of some new Vertebrate remains from the Tertiary of New Jersey; O. C. Marsh.
18. On the preservation of color in fossils from Paleozoic formations; O. C. Marsh.
19. The progress and present condition of the Geological Survey of California; J. D. Whitney.
20. The Yosemite Valley; J. D. Whitney.
21. Some points in the Surface Geology of the western side of the American Continent; J. D. Whitney.
22. The Quebec Group in Northern New Hampshire; C. H. Hitchcock.
2 . The supposed Triassic Foot-marks in Kansas; C. H. Hitchcock.
24. On the Geological age and equivalents of the Marshall Group; A. Winchell.
25. Experiment illustrating the Flow of Glaciers; Edward Hungerford
26. On the Plasticity of rocks, and origin of the Structure of the so-called Grave Stone Slates of California; W. P. Blake.
27. On the gradual Desiccation of the western portion of North America; W. P. Blake.
28. On the Physical Geography of the Continent of North America during the different Geological Periods; J. S. Newberry.
29. On two new fossil trees, the oldest known, found by Rev. H. Herzer, in the Devonian rocks of Ohio; J. S. Newberry.
30. On the surface geology of the Basin of the Great Lakes and the Upper Mississippi Valley; J. S. Newberry.
31. Fuel Resources of Illinois; A. H. Worthen.
32. Fossil Fishes, Insects, Crustacea, etc., of the Coal Measures of Grundy Co., Illinois; A. H. Worthen.
33. Geological section of Ohio; E. B. Andrews.
34. Anatomical distinction of vegetable structure rectified, followed by the circuit of generation of fresh water Algæ; T. C. Hilgard.

35. The vertebral type of the cranium a Quinary one; T. C. HILGARD.
36. On *Elasmognathus* and its relations to the Tapiridæ generally; THEODORE GILL.
37. Sketch of the Topography, Geology and Antiquities of the Caucasus; F. VON KOSCHKULL.
38. Superficial geology of the lake shore near Chicago; J. S. JEWELL.
39. On the occurrence of the Mastodon in the deep-lying gold placers of California; BENJAMIN SILLIMAN.
40. On the old lake beds of the Prairie region; S. J. WALLACE.
41. On the formation of shells and Belemnites, and Phosphates of Iron at Mullica Hill, Gloucester County, N. J.; A. B. ENGSTROM.
42. On certain physical features of the Mississippi River; G. K. WARREN.
43. The Darwinian theory of Development; CHARLES MORAN.
44. On the Interpretation of Fossils; B. WATERHOUSE HAWKINS.

The titles of 49 other papers were handed to the Standing Committee, but for want of time, a few of them only were read by title, and most of them not at all. This makes the whole number of papers presented at the meeting, 151.

2. *On the fall of Rain, as affected by the Moon;** by PLINY EARLE CHASE.—The discussion of the Moon's influence on the weather has been recently revived by European meteorologists, and an article by George Dines, Esq., in the Proceedings of the Meteorological Society, No. 36, contains some valuable tables, which seem to me worthy of special attention. Mr. Dines was entrusted with the Journal of Miss Caroline Molesworth, of Cobham Lodge, Surrey, from which he was "enabled to extract the rain which fell during each day of the moon's age, for a period extending (with few interruptions) over forty years." From these extracts he has condensed synopses for five years, ten years, twenty years, and forty years, arranged according to the several days of the moon's age, with two subordinate tables, one of which contains "groups of three or four days preceding and following each change of the moon," the other gives the proportionate amounts of rain in the successive quarters of the lunar month. There are also statements of the number of days on which ·01 inches or more of rain fell, and of the number of days on which ·25 inches or more fell, during the entire period. These summaries, as well as the data on which they are based, lead the author to the "decided opinion that the fall of rain *is in no way influenced by the changes of the moon, or by the moon's age.*"

The influence of the moon on the ocean tides, on the fluctuations of the barometer and magnetic needle, and on the winds, (see Glaisher's Tables, Proc. Met. Soc., No. 30), is so well known that the inference of a similar influence on the rainfall seems almost irresistible. I was therefore naturally startled at Mr. Dine's conclusion, and as his evident impartiality forbade any doubts of the accuracy of his results, I was led to examine into the correctness of his method. The observations cover a period of a little more than two Metonic cycles, and by the tabular arrangement the effects of the revolution of the moon's nodes, as well as those of va-

* From the Proceedings of the American Philosophical Society, June 19, 1868.

rying latitude and declination, are almost entirely eliminated.* The station is in a high northern latitude, and within the influence of the Gulf Stream, therefore the lunar modifications of the climate, especially when those modifications are estimated merely from their equatorial means, are comparatively unimportant; the four lunar quarters, each embracing an entire interval between a spring and a neap tide, are precisely the divisions which, when the periods are sufficiently extended, would eliminate most of the remaining evidences of lunar disturbance. Still, if we examine the half-cycles, or ten-year periods, we may observe that the half-months of lunar conjunction present a rainfall alternately greater and less than the half-months of opposition, a fact which suggests a possible dependence on the alternately predominating north and south latitude.

It is impossible, from the tables, to determine the extent of such a dependence. Mr. Dines, who has the necessary materials at his command, may perhaps deem the subject worthy of further investigation. The division of the month may be easily altered so as to correspond more nearly with the times of presumable maximum and minimum disturbance. If we regard the day of each change of phase as the middle day of a week, (counting the half-sum of the 5th and 12th days in the first quarter, and the half-sum of the 20th and 27th days in the third quarter), the seven days' aggregates in Table 1, and in the two summaries on pp. 136–7 (*loc. cit.*), will furnish the following results:

Aggregates of Tabular Ratios and Number of Rainy Days.	Week of New Moon.	Week of First Quar.	Week of Full Moon.	Week of Last Quar.	Two Weeks of Syzygy.	Two weeks of Quadrature.
1825 to 1829,	770	606	679	766	1449	1372
1830 to 1834,	657	797	599	731	1256	1528
1835 to 1839,	598	800	668	617	1266	1417
1840 to 1844,	643	708	760	662	1403	1370
1845 to 1849,	830	726	602	627	1432	1353
1850 to 1854,	687	726	614	810	1301	1536
1855 to 1859,	507	722	728	735	1235	1457
1860 to 1864,	706	680	679	754	1385	1434
1825 to 1834,	716	698	638	750	1354	1448
1835 to 1844,	618	753	771	639	1389	1392
1845 to 1854,	765	724	608	711	1373	1435
1855 to 1864,	651	701	702	744	1353	1445
1825 to 1844,	667	726	704	693	1371	1419
1845 to 1864,	708	714	656	729	1364	1443
1825 to 1864,	687	721	681	709	1368	1430
·01 inch or more,	1490	1484	1446	1511	2936	2995
·25 inches or more,	260	268	250	272	510	540

* Lubbock found a barometric elevation of nearly ·1 inch for 17° increase of declination. *Phil. Trans.*, 1841, p. 73. He appears to have been led to the investigation by Howard's remarks "on a cycle of eighteen years in the mean annual height of the barometer in the climate of London." *Clim. of London*, 2d ed., v. i, p. 172; *Phil. Trans.*, 1841, p. 277, seq. See also Zenger's discussion of the mean annual temperature, as affected by the revolution of the moon's nodes and apsides. *Phil. Mag.*, v, 35, June, 1868.

It may thus be seen that, *notwithstanding the complete veiling of all the disturbances which may be due to the moon's variable distance and declination, there was a marked tendency to increase at quadrature and to decrease at syzygy, both in the amount of rain and in the number of rainy days.* This tendency, which becomes evident even in the majority of the five years' groupings, is uniformly shown in all the groups of ten years, twenty years, and forty years, as well as in the number of rainy days and in the number of heavy rains during the entire period.

In a future communication I propose to discuss the observations at Pennsylvania Hospital, which demonstrate the existence of similar tides at Philadelphia. The forty years' aggregates (1825 to 1864, inclusive, at each station) exhibit the following ratios of weekly rainfall:

	Surrey.	Philadelphia.
Week of new moon,	98·2	97·6
" first quarter,	103·1	100·3
" full moon,	97·4	95·8
" last quarter,	101·4	106·3

The tide is so strongly marked at Mussoorie, on the southern range of the Himalaya Mountains, as to be strikingly shown by 13 years' observations (1854–1866) on the days of change. (See Mr. Hennessey's communication, Proc. Roy. Soc., v, 16, December 12, 1867.) The mean results are,

	Average daily fall.	Ratios.
Day of new moon,	·402	86·2
" first quarter,	·535	114·7
" full moon,	·399	85·6
" last quarter,	·529	113·5

3. *On a supposed connection between the amount of Rainfall and the changes of the Moon*, being an extract of a letter from J. H. N. Hennessey, Esq., First Assistant on the Great Trigonometrical Survey of India, to General Sabine, R.A., Pres. R.S., (Proc. Roy. Soc., xvi, 213.)—Allow me now to say a few words in connection with the inclosed paper. There appears to prevail a belief, more or less popular, to the effect that more rain falls at "the changes of the moon" than on the intermediate days of a lunation. As I happened to possess a record of the rainfall at the office of the Superintendent of the Great Trigonometrical Survey of Mussoorie, extending over thirteen consecutive years, I obtained Colonel Walker's permission to make use of the Register, in connection with this popular belief.

The results tabulated have been obtained by employing an *average daily* fall as the means for comparing the fall *at* "the changes" with that at intermediate intervals. The method of calculation adopted is explained in the foot-note to the Table. The annual average result may be stated thus:—

	Inch.
At "the changes" of the moon the *mean daily* fall of rain is	0·466
Between "the changes" of the moon the mean daily fall is	0·525

which is in opposition to the popular belief on the subject. I inclose the Table, on the chance of its proving sufficiently interesting to be noticed.

Average daily fall of rain between successive quarters and at each quarter of the moon from 1st of May to 31st of October of each year, measured at the office of the Superintendent of the Great Trigonometrical Survey of India. The office stands in Mussoorie, on the most southern range of the Himalaya Mountains, lat. N. 30° 28′, long. E. of Greenwich 78° 7′; height above mean sea-level 6500 feet.

Year	Average Daily Fall.								Total Fall from May 1 to Oct. 31.
	◑ to ●	●	● to ☽	☽	☽ to ○	○	○ to ◑	◑	
	inch.	inch.	inch.	inch.	inch.	inch.	inch.	inch.	inches.
1854	·644	·374	·813	·176	·630	·096	·512	·621	100·72
1855	·456	·204	·360	·918	·311	·356	·753	·733	85·85
1856	·732	·745	·703	·237	·397	·588	·347	·340	93·28
1857	·280	·319	·794	1·013	·521	·136	·368	·606	88·27
1858	·402	·448	·485	·298	·518	·157	·705	·373	84 61
1859	·665	·263	·253	·642	·306	·253	·570	·583	78·31
1860	·356	·228	·430	·719	·564	·205	·301	·073	65·81
1861	·685	·678	1·014	·372	1·332	·287	·577	·855	141·16
1862	·611	·620	·513	·651	·364	·852	·645	·530	93·91
1863	·348	·342	·862	·932	·511	·595	·291	·546	93·03
1864	·762	·409	·545	·292	·394	·328	·237	·352	82·19
1865	·543	·235	·276	·120	·443	·526	·518	·785	76·37
1866	·135	·360	·402	·580	·636	·809	·452	·483	81·15
Means of columns	·509	·402	·573	·535	·533	·399	·483	·529	89·589

General mean of ● ☽ ○ ◑ ..... 0·466 inch.
General mean of ◑ to ●, ● to ☽, ☽ to ○, ○ to ◑ .... 0·525 "

Note.—The rainfall during the *preceding* twenty-four hours was measured daily at mean noon. Suppose m_1, m_2, m_3, m_4, m_5, m_6, m_7, m_8, m_9, to denote nine such consecutive measurements of daily rainfall, registered at Mussoorie mean noon, respectively on the 1st, 2d, . . . 9th of the month, and that the moon entered her first quarter at an hour nearer to noon of the 1st than to the preceding or succeeding noons. In this case the arithmetical mean of m_1 and m_2 has been entered in column ☽ as the average *daily* fall *at* the first quarter. Similarly, if full moon occurred nearest to noon of the 8th, the quantity $\frac{m_8+m_9}{2}$ has been reckoned as the average *daily* fall *at* full moon; and $\frac{m_3+m_4+m_5+m_6+m_7}{5}$ represents the average *daily* fall *from* ☽ to ○. The foregoing Table has been prepared under these conditions by Baboo Dwarkanath Dutt, Computor to the Great Trigonometrical Survey of India.

4. *On a new Meteorite.*—H. B. Geinitz has described a new meteorite, handed to him for examination in September last by Pastor Nürnberger of Nöbdenitz, near Schmölln, duchy of Altenburg, who found it about a foot below the surface of the ground in digging a ditch near his dwelling. It was irregularly hexagonal in shape, and appeared to be a portion of a much larger stone. In its longest diameter it measured 10·5 centimeters, in breadth 9 centimeters, in thickness from 2–5 centimeters. Its surface was

covered with the characteristic black, sometimes dark brown crust of ferric oxyd and hydrate, with here and there traces of tile ores and malachite. The entire mass weighed 1·2194 kilograms; with the greatest difficulty it was broken into fragments, the largest of which, weighing 976 grams, was, together with some smaller fragments, retained by Pastor N., a second piece, 163·75 grams in weight, was deposited in the Royal Museum in Dresden. The structure of this fragment is compact, it is divided only with difficulty, is slightly malleable, strongly magnetic, and possesses a finely granular fracture, which when fresh, exhibits a light steel-gray color. The hardness is from 5–6; gravity of the interior portion, 7·06, and of fragments with the crust, 6·75 and 5·8 (Fleck).

The analysis was made by Dr. Fleck. After removing a trace of insoluble residue, in which silica was recognized, the fresh interior material afforded:

Iron,	88·125
Copper,	9·013
Nickel,	1·340
Tin,	1·321
Cobalt and Chromium,	traces.
	99·799

—*Ber. Soc. Isis zu Dresden, Nov.* 14, 1867.

OBITUARY.

Professor Matteucci.—The Italian papers record the death of Professor Matteucci on Friday morning last, at Florence, after a short illness. The deceased was an Italian senator and Minister of Public Instruction, in which capacity he was very active in promoting the extension of education. But he was better known as a man of science than as a politician or a minister. He obtained, in 1844, the prizes of the French Academy of Sciences and the Copley Medal of the Royal Society for his investigations in electro-physiology. His "Lectures on Physics" passed through four editions. He published also "A Manual of Telegraphy," "A Treatise on Electro-physiological Phenomena," "Elements of Electricity as applied to the Arts," and "Lectures on the Physico-chemical Phenomena of Living Bodies," which has been translated into English and French.—*Chemical News.*

VI. MISCELLANEOUS BIBLIOGRAPHY.

1. *Cambridge Physics: A Hand-book of Natural Philosophy;* by W. J. Rolfe and J. A. Gillet, Teachers in the High School, Cambridge, Mass. Boston, 1868, (Woolworth, Ainsworth & Co.) —This is a School Text-book, based on the freshest authorities, clear of traditional rubbish, and presenting, as we judge from a cursory examination, more excellencies and fewer defects than any of the school philosophies in use. It consists of two distinct works, or parts, bound together: the first, the elements of Natural Phi-

losophy under the three heads, Pressure, Motion, and Machines and Sources of Mechanical Power; the second, Sound, Light, and Heat, with an appendix on Energy. Electricity and Magnetism are omitted, because embraced in another volume of the same series on chemistry.

Full explanations of difficult points are not to be expected in an elementary work; yet, if attempted at all, they should not, in our view, be so defective and inadequate as is that of the difference between Newton's theoretical and the observed velocity of sound, p. 8, part II.

The book is well furnished with illustrations, problems, questions, and indexes. C. S. L.

2. *Braithwaite's Retrospect of Practical Medicine and Surgery.* Part LVI, January, 1868. New York. (W. A. Townsend & Adams.)

Ranking's Half-Yearly Abstract of the Medical Sciences. Vol. XLVI, from July to December, 1867. Philadelphia. (Henry C. Lea).—These semi-annual digests of the current medical literature of the world, reprinted from the London editions, come to us at irregular intrevals. The former contains a hundred and fourteen articles, the latter, two hundred and sixty eight; presenting in a small compass, and for a small price, an abstract of the most important papers published within the last six months, in the various departments of the medical sciences.

We know of but one valid objection to these publications, which have become so indispensable to the busy practitioner,—their contents are so valuable and complete, and conveniently arranged, that in many cases the temptation to depend entirely upon them, to the exclusion of a wider reading of medical authors, may not be successfully resisted.

3. *Archives du Musée Teyler*, vol. I, Fascicule 1, 2, 3, Harlem, 1866, 1867, 1868. Roy. 8vo.—The Archives of the Teyler Museum are issued by the Directors of the Teyler foundation, and are to appear from time to time as memoirs may be ready for publication. The papers in the three parts thus far issued, are elaborate, and pertain to questions in Physics and Chemistry; the subjects are the following: On the determination of the lengths of the wave of the Solar Spectrum, by M. S. M. Van der Willigen; On the refraction and dispersion of flint glass, by the same; On the determination of the indices of refraction and on the dispersion of mixtures of sulphuric acid and water, by the same; On a case of explosion by illuminating gas, by the same; On the indices of refraction of some saline solutions and two other liquids of feeble dispersion, by the same; On the refraction of water, by the same. The Archives contain also a continuation of the catalogue of the library and of the museum.

4. *The American Naturalist*, Vol. II, No. 5, July. *A Sea-side number.* Salem, Mass.—The midsummer number of this excellent magazine is very appropriately devoted exclusively to marine Zoölogy and Botany, which will make it a very useful and interesting work for those who visit the sea-side summer resorts. It contains,

Sea-Weeds, by J. L. Russell; A Stroll by the Sea-side, by E. S. Morse (illustrated); Our Sea-Anemones, by A. E. Verrill; The Marine Aquarium; A few Sea-Worms, by A. S. Packard (illustrated); Good Books for the Sea-side; Insects living in the Sea, by A. S. Packard (illustrated); Directions for Collecting Marine Animals, etc.

5. *Reliquiæ Aquitanicæ;* being contributions to the Archæology and Palæontology of Perigord and the adjoining Provinces of Southern France, by E. LARTET and H. CHRISTY: edited by Thomas Rupert Jones. Part V. Pages 33–52, and 73–80. Plates A, xv, xviii, and Sketches of the Vezere, Nos. 1 and 2. London, April, 1868. (H. Baillière).—This new part, like the preceding, contains numerous excellent figures of flint arrow heads, chippings, etc., besides a page of cuts representing North American Implements of stone, metal and bone, for comparison with the more ancient implements of Europe.

6. *A treatise on concentration of all kinds of ores, including the chlorination process for gold-bearing sulphurets, arseniurets, and gold and silver ores generally:* by GUIDO KUSTEL, Mining Engineer and Metallurgist, author of "Nevada and California Processes of Silver and Gold Extraction." 260 pp. 8vo, with 120 Diagrams on 7 plates. San Francisco, 1868. (Office of the Mining and Scientific Press.) We defer a notice of this important work to another number.

7. *Catalogue de Minéraux, Roches, Fossiles, Modèles en Platre et Modèles en Bois des cristaux à vendre dans le Comptoir Minéralogique Rhénan,* (à Bonn) du AUGUSTE KRANTZ, Dr. der Sci. ix ed., 1868.—A new edition of Dr. Krantz's Catalogue, showing extensive collections of minerals, rocks, fossils, casts, etc., the largest in fact in the world, and also very reasonable charges.

Proceedings of the American Association for the advancement of Science, 16th meeting, held at Burlington, Vermont, Aug. 1867. 178 pp. 8vo. Cambridge. 1868. Published by Joseph Lovering, Permanent Secretary.

A system of instruction in the practical use of the Blowpipe. 2nd ed., with an appendix and index, by G. W. Plympton, A.M. 288 pp. 12mo. New York: (D. Van Nostrand.)

Discussion of the West India Cyclone of Oct. 29, 30, 1867. Prepared by order of Commodore B. F. Sands, U. S. N., Superintendent U. S. N. Observatory, by J. R. Eastman, Prof. Math., U. S. N. 18 pp. with a map. 8vo, Washington, 1868.

A Dictionary of Chemistry, and the allied branches of other sciences, by Henry Watts, B.A., vol. v, completing the work. London, 1868: (Longmans).

Elemente der Mineralogie von Dr. Carl Friedrich Naumann, Prov. Univ. Leipzig, etc, 7th ed., 1st part with numerous illustrations. Leipzig, (W. Engelman)

Lehrbuch der physikalischen Mineralogie, von Dr. A. Schrauf. vol. 2nd, 426 pp. 8vo, with numerous illustrations. Vienna, 1868: (W. Braumüller).

Atlas of charts of the Meteor tracks contained in the British Association Catalogue of observations of Luminous Meteors, extending over the years 1845 to 1846; showing the principal dates of apparition, the limits of duration, the positions of the radiant points, the distinctive appellations (by Heis and Greg), and the specific appearances of the meteors of all the general and special meteoric showers hitherto observed in the northern hemisphere throughout the year. Prepared for the Luminous Meteor Committee of the British Association by R. P. Greg and A. S. Herschel. 23 plates, folio, 1867.

Report of the Astronomer Royal to the Board of Visitors of the Royal Observatory, Greenwich, June 6, 1868.

Catalogo degli Uranatmi (ossia stille Cadenti) &c.; catalogue of shooting stars observed at Rome in the years 1861–1867 by Madame Caterina Scarpellini. 4° 16 pp. Rome, 1868.

Vierteljahrschrift der Astronomischen Gesellschaft, I and II, Jahrgang, 8°. 1866 and 1868. Leipzig, (W. Engelmann.)

Publicationen der Astronomischen Gesellschaft, I—VI, VIII. 4. 1865—1866, brosch.

I. Hülfstafeln zur Berechnung specieller Störungen, enthaltend die rechtwinkligen Ekliptical-Coordinaten und die vom Orte des gestörten Körpers unabhängigen Theile der störenden Kräfte für die Planeten Venus, Erde, Mars, Jupiter, Saturn, Uranus und Neptun von 1830–1864. 1865.

II. Lesser, Dr. Otto, Tafeln der Metis, mit Berücksichtigung der Störungen durch Jupiter und Saturn. 1865.

III. Weiler, Dr. A., über das Problem der drei Körper in Allgemeinen und ins Besondere in seiner Anwendung auf die Theorie des Mondes. 1866.

IV. Hoüel, Dr. G. J., Tables pour la reduction du temps en parties décimales du jour. 1866.

V. Auwers, Arth., Reduction der Beobachtungen der Fundamentalsterne am Passageinstrument der Sternwarte zu Palermo in den Jahren 1803–1805 und Bestimmung der mittleren Rectascensionen für 1805. 1866.

VI. Rechtwinkelige und Polarcoordinaten des Jupiter (nach Bouvard's Tafeln), sowie Componenten der störenden Kräfte, mit denen Jupiter auf die Sonne wirkt von 1770—1830. 1866.

VIII. Schjellerup, Genäherte Oerter der Fixsterne von welchen in den astronomischen Nachrichten Band 1—66 selbständige Beobachtungen angeführt sind für die Epoche 1855 hergeleitet und nach den geraden Aufsteigungen geordnet. 1867.

Travaux Publiés et constructions civiles; Rapports du Jury international reunis par ordre de son Excellence M. de Forcade la Roquette, Ministre de l'agriculture, du commerce et des Travaux publics. 458 pp. 8vo. Paris, 1868.

Proceedings Boston Soc. Nat. Hist., vol. xii.—Page 1, Report of the Custodian, additions to the Library and Museum, Trustees accounts. On page 75 the officers for the ensuing year are stated as follows: President, Jeffries Wyman; Vice Presidents, C. T. Jackson, T. T. Bouvé; Corresponding Secretary, Samuel L. Abbot; Recording Secretary, S. H. Scudder; Treasurer, Edward Pickering; Librarian and Custodian, S. H. Scudder. Page 77, further enumeration of New England Fungi; *C. C. Frost* of Brattleboro, Vt.

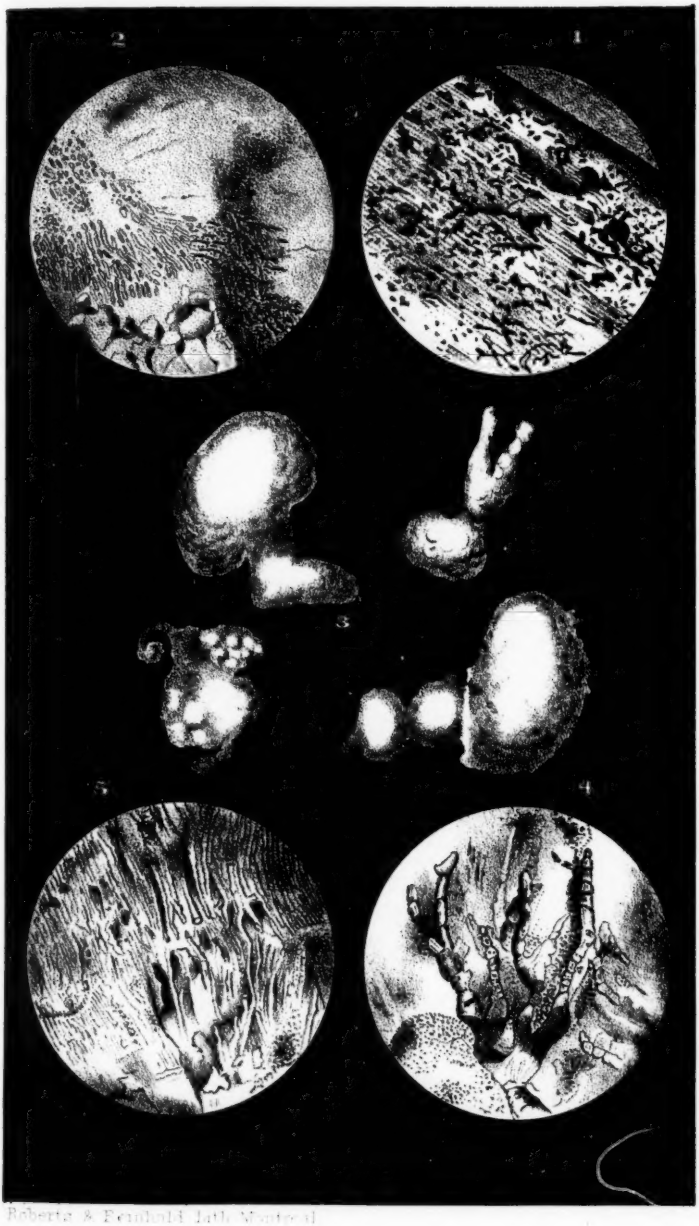
Trans. Acad. Sci. St. Louis, vol. II.—Page 459, North American Species of Juncus (continued); *Engelmann.*—p. 499, Account of the passage through the great Canon of the Colorado, by Mr. James White, in 1867, with Geological Notes; *Parry.*—p. 504, Age of the Porphyry Hills of southeast Missouri; *Harrison.*—p. 507, Mr. Meek's notes on his preliminary Report of the Geology of Kansas; *Swallow.*—p. 526, Yearly report of Atmospheric electricity, temperature, and humidity for 1867; *Wislizenus.*—p. 532, On the character of the persistent snow accumulations in the Rocky Mountains, and certain features of the Alpine Flora; *Parry.*—p. 557, Analysis of the Rock Salt of Louisiana; *Sander.*—p. 561, Observations of Meteoric Showers; *Hayes.*—p. 562, On the dyas in Nebraska, (translation by N. Holmes); *Marcou.*—p. 565, Remarks on the Loess and Drift of Missouri and Illinois, and upon the Big Mound at St. Louis; *Holmes.*

Transactions of the American Entomological Society, Vol. II, No. 1.—Page 1, Catalogue of a collection of Hymenoptera from Mexico; *E. T. Cresson.*—p. 49, New Coleoptera collected between Kansas and Fort Craig, New Mexico, *J. L. LeConte.*—p, 67, Notes on the N. A. Lepidoptera in the British Museum, and described by Mr. Francis Walker; *A. B. Grote* and *C. T. Robinson.*—p. 89, List of the Ichneumonidæ of North America, with descriptions of new species; *E. T. Cresson.*

Proceedings Essex Inst., Vol. v, No. vii.—Page 233, Flora of the Hawaiian Islands; *H. Mann.*—p. 249, Catalogue of the Birds contained in the Museum of the Essex Institute; *Elliot Coues.* Naturalist's Directory, Part II, North America and the West Indes; Pages 57–64; Mollusks concluded, Radiates, Protozoans, Parasites, and additional names in the departments already reviewed.

Roberts & Reinhold Lith. Montreal.

EOZOON CANADENSE.



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